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NOTES AND COMMENTS

"What's in a Name?"

Some apology will doubtless be demanded by our readers for that we present them, within a single number, with two lengthy articles on special points of zoological nomenclature. We venture to think, however, that the articles appeal to wider circles than the select few who pose as specialists on crinoids and crayfish. The animals whose names are in question are among the best known of invertebrates; of some it might be said that they are in everybody's mouth (for which remark we offer further apology). Is it not a little humiliating to zoologists that they are still undecided what to call a lobster? It is not even safe to call it a Crustacean; for, if some priority-fanatics had their way, the name Crustacea would be the heritage of what are now known as echinoderms. It is doubtful whether obedience to any code of rules could effect the automatic solution of all these puzzles; certainly their settlement needs a far wider and deeper knowledge of ancient literature than is possessed by any living zoologist. Each day almost brings its discovery and the second-hand bookstall reverses a time-honoured belief. Let us pray that the speedy adjustment of these disputes may set free brains and energy to find out a little more about the facts of nature, and that the present reign of topsy-turvydom may be remembered only in some modern "Stultifera Navis," towards which our parodist makes his humble contribution:—

The lobster and the crayfish
  Were walking claw in claw,
They wept like anything to hear
  Such quantities of jaw.
If only this were cleared away,
  They said, we should guffaw.

"Mutato Nomine——" Date

It may be remembered that in *Natural Science* for August 1896 we endorsed a suggestion made by Prof. Herrera, that the specific name should be followed, not by the name of its author, as is usual, but by the date of its first publication. We are interested, therefore, to find from a paper sent us by Dr K. Jordan, that the proposal has been put into practice by that distinguished systematist in describing a collection of Coleoptera belonging to the family *Anthribidae* (*Ann. Mus. Civ. Storia Nat. Genova*, Ser. 2a, vol. xviii., pp. 623-643). Dr Jordan's reasons for adopting this course are thus expressed by him: (1) "While the name of the author does not help me much, if there is no full reference given, to find out when and where the insect in question has been described, the addition of the year to the name of the insect tells us at once in which volume of the *Zoological Record* the place of the description can be found.

(2) "As I am the 'author' of about 300 species of *Anthribidae*, and shall probably become the 'author' of a still larger number, I should have to bring my own name before the reader again and again, and thus acquire that kind of cheap immortality to which critics of classificatory work have nowadays so often alluded as being the chief aim of the publication of mere descriptions of species. By the above method of citing the name of a species this cutting criticism loses the point." Example is always better than precept, and we hope that this one will find many followers.

Professor Liversidge's Address

Professor Liversidge, as President of the Australasian Association, dealt chiefly in his address with the history of the movement for the advancement of science. He referred to the origin of the British Association in 1831 at the instance of Sir David Brewster, who had for objects the wish to give a strong impulse and a more systematic direction to scientific inquiry; to bring together British and Foreign philosophers; and to obtain a greater degree of national attention to the objects of science and a removal of any disadvantages of a public nature which impeded its progress. Prof. Liversidge referred to the peripatetic character of the British Association and the system of making grants to individuals, a system productive of valuable results; and then passed on to the origin in 1884 of the Australasian Association, conducted on precisely similar lines. He reviewed the results of previous sessions; discussed the value of the proposed International Catalogue of Scientific Literature "if carried to a successful issue"; and referred to the Imperial Institute in London as "now fulfilling its intended objects," a statement which will be

hugged to the breasts of those responsible for this happy condition of affairs. Then passing to more familiar ground, the President dealt with the chemistry of the ancients as recently discussed by Prof. Berthelot; passed an eulogy on the London County Council, which views a little chemistry pure and simple as a valuable means of education, and expressed the hope that Australia would follow suit. The conclusion of his address was taken up with a review of Argon and Helium, and the recent remarkable advances in the liquefaction of gases.

Other Australian Addresses

Professor Hutton, the President of the Geological and Mineralogical Section, took for his theme "The early life on the Earth," imagining that the first living organisms were evolved near the surface of a warm ocean, which contained abundance of hydrocarbons and atmospheric air in solution, and which was agitated by wind and other meteorological agencies. Plants formed the army of invasion that conquered the land. Prof. C. J. Martin, who, on the lamented death of Prof. Jeffrey Parker, undertook the delivery of the Presidential Address on Biology, dealt with the relations of morphology and physiology. We are unable to give the points of his address, as neither of our authorities report it owing to its "highly technical character." Sir James Hector, as President of the Geographical Section, called attention to the great advances made of recent years in submarine geography, laying particular stress on the Nansen expedition, the results of which he summed up, while waiting for the full scientific reports, as follows:—That the North Polar Ocean was not a shallow sea with scattered islands distributing icebergs, but a profound ocean basin; and that there were definite movements of the great ice-cakes, which crossed and did not merely circulate round the Pole. He also referred to the great value of the Elder and Horne expeditions to Central and Western Australia, and the steady and considerable extension of the Artesian well system, which was of great promise to the future advancement of Australia. Mr R. H. Mathews, in the section of Ethnology, dealt with the various pictographs of the Australian aborigines and with Australian initiation ceremonies. The Government statist of Tasmania, Mr R. M. Johnston, President of Economic Science, took as the subject of his address "Consumable Wealth."

Earthquakes in Australia

Mr George Hogben, in presenting the report of the Seismological Committee to the Australasian Association, referred to the work

being done by the Telegraph Department of New Zealand, who, on the occurrence of any earthquake shock, fill up certain forms stating the exact time, duration, and other details of such shocks as occur in their own region. This information is of much value, and has been collected in New Zealand since 1889, and though some of the other colonies have instituted similar observing stations, Mr Hogben asks for a more uniform and developed system. He asks also for the immediate establishment of stations at Sydney, Melbourne, and Timaru, in order that a chain of stations might be established which would then be continuous around the world.

The Origin of the Australian and Tasmanian Aborigines

Mr Howitt, the President for Ethnology, in dealing with the origin of the aborigines, said he was of opinion that, in spite of the contention of many writers to the effect that the primitive Australians and Tasmanians had come from other lands in ships or canoes, there were but few evidences to show that they had any knowledge of navigation or of sea-going vessels. Whatever evidence there was in the customs or in language of the aborigines, from the time that the first voyagers visited these shores, led to the conclusion that their ancestors knew nothing better than the catamarans of the Tasmanians or the bark canoes of the Australians. In any theory as to the origin of the natives of Australia, one fundamental element must be that the ancestors of these savages reached Australia or Tasmania by land; or if the land connection was not continuous, the intervening channels were such as could be traversed by vessels no better than the catamarans or canoes above mentioned. He quoted many geological facts, all of which tended to show that an immense period of time was one of the elements of any solution of the problem, and that during that period the Australians had been isolated from outside influence, having at the same time a continental area in which to develop their institutions. The level of culture of the Tasmanian had been termed the eolithic, and that of the Australians might fairly be termed neolithic, or even as regards some of the tribes of Central Australia bordering on the palaeolithic. The social organisation of the Tasmanians was also below the level of that of the Australians. Mr Howitt came to the conclusion that the Australians reached their continent by a land bridge connected with the Indo-Asiatic continent, or by a land extension of the Austral continents to the north-west, or over some shallow channels separating Australia from these lands.

Artesian Water in Australia

The construction of Artesian wells all over the arid districts of Australia, to which we have often referred in these pages, bids fair to develop the resources of the continent in a remarkable manner. The Rev. J. Milne Curran read an interesting paper on the subject before the geological section of the Australian Association, in which he briefly sketched the history of Artesian exploration. Referring to the Ballimore, Trangie, Coonamble, and Dubbo bores and the cores brought up from the 1896 bore at Salisbury Downs, the author of the paper concluded that the Artesian water of the north-west and west of New South Wales was derived from Triassic strata, and that there was no evidence to show that a single Artesian well in New South Wales derived its waters from Cretaceous rocks. Mr R. L. Jack, in the discussion which followed, stated that Artesian water had just been struck in Queensland, on the central line of railway at a place called Maria Creek, west of Dawson River, in rocks of Permo-carboniferous age. These are the oldest rocks in which Artesian water has been struck in Australia.

The Aborigines of Jamaica.

Vol. II. of the *Journal of the Institute of Jamaica* (July 1897) contains a paper by Mr J. E. Duerden, curator of the Museum of the Institute, on the "Aboriginal Indian remains in Jamaica." From this we may gather that the antiquities of the island are being investigated with zeal and studied in a systematic manner, and that endeavours are being made to collect material for the formation of a really good museum of local antiquities. Chapter I. deals with the kitchen-middens, refuse and shell-heaps, of which a large number have already been discovered and partially explored. From them much may be learnt as to the life of the Indian inhabitants in pre-Spanish times, a culture which waned rapidly under Spanish domination. A brief sketch is given of the individual finds of this class and of the objects found therein; and, while no great detail is as yet forthcoming, the preliminary investigation will, no doubt, lead to more systematic and exhaustive research. The Burial Caves are treated of in Chapter II. The prevalence of cave-burial in Jamaica was due to presence of very numerous natural caves and sink-holes in the 'White and Yellow Limestone' formation which covers a large proportion of the island of Jamaica. The human crania found in these caves shew in all, or nearly all, instances traces of artificial deformation, very marked in some cases. Examples of pottery vessels, which in the kitchen-middens are, as might be expected from the nature of the deposits, always in a fragmentary condition, have been found in a

perfect state in the caves; no evidence of the use of even a rudimentary potter's wheel is forthcoming. Stone and shell implements necessarily form an important series referred to in the paper, but the forms described are for the most part well known, though a few rarities are recorded. The paper also deals with the human-form grotesques, of which a few have been found in the island, and refers to a few interesting rock carvings and petroglyphs which remind one of examples from other islands of the West Indies and from the mainland.

The map at the end, showing clearly the position of the already ascertained ancient remains, is well done, and should prove very useful. We can heartily wish success to the Museum authorities in their work of collecting and collating the available local material. It is to be hoped that collections both from the other neighbouring islands and from the mainland region of Guiana may be associated with the Jamaica finds, in order that further light may be thrown upon the past history of a most interesting race. If this is well carried out, this museum will have fulfilled a duty of very high importance.

A Parallel between Chemical and Vital Phenomena

When a saturated solution of a crystalline substance is allowed to cool below the saturation point, the dissociated molecules dissolve their partnership with the fluid and slowly build up the structures we know as crystals. There are, however, a number of curious conditions of this process. In some cases, if the solution be kept absolutely still, no crystallisation occurs, but a slight jar, or stirring with a wand, apparently miraculously transforms the liquid into a mass of crystals. In other cases, it is necessary, or at least advantageous, that some foreign granules of dust, or threads of cotton be present, to serve as nuclei around which the forming crystals cluster, as the layers of nacre in a shell-fish transform an intruding grain of sand into a radiant pearl. But most curious of all are the cases in which an almost necessary stimulus is the presence of an already formed crystal of the crystalline salt. The analogy between this and the processes of life stares at one. One fluid may contain all the ingredients for the building of crystals, but the crystals refuse to form until a formed crystal is dropped into them; another fluid may contain all the necessary food materials for the building of protoplasm, but it remains barren of life until a spore, a tiny mass of protoplasm, has reached it, and then at once the building of protoplasm begins and proceeds apace. Such comparisons have been made, and are worth making, if it be remembered that they are things of the dreamland, of the after-dinner meditative hours of

science, rather than children of the working hours. None the less, we notice in the *Revue Rose* (1898, p. 116) that Prof. L. Ranvier has made the comparison between the stimulation of crystallisation by crystals and the regeneration of the membrane of Descemet. Immediately under the cornea of the eye there lies a thin, tough membrane named after Descemet. In embryological development it arises as a secretion from a layer of endothelial cells lying next it on the inner side. Prof. L. Ranvier has observed the process of its reconstruction after operative or accidental interference. In such cases, too, it arises as a secretion from the endothelial layer, but he says the reconstruction begins around the edges of the undestroyed portions of the membrane, and creeps only slowly to the centre of areas over which the old membrane has been entirely lost. He regards the process as showing that the edge of the undamaged membrane stimulates the adjacent endothelial cells to the formation of new membrane, as a formed crystal stimulates crystallisation.

The Growth of Coral Islands

We are not surprised to find that Dr John Murray holds a different opinion as to the conclusions to be drawn from the boring on the coral island of Funafuti from those which we expressed last November. In a lecture to the Edinburgh Geological Society on November 19th, Dr Murray maintained that the boring confirmed his own theory. He has explained the presence of deep water alongside coral reefs by the supposition that blocks of dead coral fall down and form a steep-sloped talus on which living coral can grow within the limit of depth necessary to its existence. The boring on Funafuti is believed by Dr Murray to have been sunk through such a talus. He lays stress on the admission that the atoll has recently been raised about 4 feet, and that the reefs are extending seawards. Those who are interested in this subject should read what is said in our last number by Mr Charles Hedley, who was the representative Australian zoologist on the first expedition.

We may also draw attention to a remarkably interesting letter on the coral reefs of the Fiji Islands, communicated by Prof. Alexander Agassiz to the February number of the *American Journal of Science*. Prof. Agassiz, who has for some months been exploring the Fiji Islands in company with Dr W. M'M. Woodworth and A. G. Mayer, considers that they do not present that strong evidence in favour of the Darwinian explanation that J. D. Dana and Darwin himself supposed. The shape of the atolls and of the barrier reefs, he writes, "is due to causes which have acted during a period preceding our own. The islands of the whole

group have been elevated, and since their elevation have, like the northern part of Queensland, remained nearly stationary and exposed to great and prolonged denudation and erosion, which has reduced the islands to their present height; the platforms upon which the barrier-reef corals have grown being merely the flats left by the denudation and erosion of a central island of greater size than that now left; while the atolls are similar flats from the interior of which the islands have been eroded and the lagoons of which have been continually scoured by the action of the sea, the incessant rollers pouring a huge mass of water into the lagoon, which finds its way out through the passages leading into it." In short, the Fijian area is one of elevation and not of subsidence, though what the age of the elevated reef may be is uncertain. Since it attains a thickness of 800 feet, it was very probably deposited originally during the period of subsidence, but this has nothing to do with the present shape of the atolls. Prof. Agassiz, who is informed by Prof. David that the evidence of the Funafuti boring was not so simple as was at first supposed, suggests that the boring merely penetrated the base of an ancient reef, and therefore in no way corroborates the theory of a subsidence. Prof. Agassiz is also inclined to ascribe the formation of atolls more generally than has hitherto been done to the erosion of volcanic summits or of extinct craters. He describes one or two undoubted cases in the Fiji group, and points out that many of the smaller atolls may have been formed in the same way. The great depth of the lagoons of some of the atolls is quite as intelligible upon this view as on the theory of subsidence. All interested in coral reefs will look forward to the fully illustrated report of his cruise which is promised by Prof. Agassiz.

A Note on Plant Distribution

We recently received a number of a bi-monthy French journal, probably unknown to most of our readers, the *Revue des Sciences Naturelles de l'Ouest* (vol. vii., No. 2), which contains the second and concluding part of an account of the genus *Acaena* by M. P. Citerne of Nantes. *Acaena* is a genus of Rosaceae, very closely allied to *Poterium*, two species of which, *P. Sanguisorba* (Salad Burnet) and *P. officinalis* (Great Burnet), are well-known members of our British flora. It resembles *Poterium* in habit, containing about forty species of small low-growing herbs or under-shrubs. *Poterium* is a north temperate plant; *Acaena*, on the other hand, is widely spread over the southern hemisphere, advancing in America as far north as Mexico. Associated with its occurrence over so wide an area we find an interesting means for distribution in the structure

of the fruit. This is small, and, as frequently happens in Rosaceae, is a pseudocarp (like the apple) composed of the somewhat swollen floral axis, enclosing the true fruit with a single seed. In shape it is rounded, oval, or pyramidal, generally with a narrowed base, and bears outgrowths in the form of spines, or more rarely wings. The spines appear before flowering, and develop and often increase in number as the fruit ripens; they vary considerably in number, size, and shape, and often end in a barb, like a fish-hook. They are thus eminently adapted for clinging to the fur of animals or plumage of birds, and the wide distribution of the genus and its presence on isolated Oceanic islands can be accounted for by the transport of the fruits by widely-ranging sea-birds. It is of interest to notice that M. Citerne, in his division of the genus into seven sections, relies to a great extent for sectional characters on the number and form of these outgrowths and their distribution over the surface of the fruit. The chief centre of distribution, where, that is, the genus is most richly developed, is in extra-tropical South America. Nine of the thirty-eight species are endemic in Chili, four spread southwards through Patagonia to the Straits of Magellan and Tierra del Fuego, one is confined to Patagonia, one to Tierra del Fuego. One species occurs only in the Straits of Magellan, Tierra del Fuego, and the Falkland Islands, another in these three localities, and is reported from Mexico; a third in the same three, and also in New Zealand. The Falkland Islands, and the Islands of St Paul and Amsterdam in the South Atlantic, have each a peculiar species, while one, the most widely distributed of the genus (*A. sanguisorbae*), is found in Tasmania, New Zealand, at the Cape of Good Hope, and in the Island of Tristan da Cunha and the Campbell Islands. One species is confined to the Cape. A small section of three species, characterised by fruits without spines, is confined to New Zealand, and three others occur only in Australia (with Tasmania). Tracing its path northwards we find a few species in the Andes of Peru, Bolivia, Columbia, and New Grenada, two of which reach as far north as Mexico, while one is peculiar to the Sandwich Islands.

Japanese Explorers in Formosa

In our January number, we alluded to the geological exploration of Formosa now being undertaken by the Japanese. We learn from the *Révue Scientifique* that the highest peak of that island, Mount Morrison, was ascended for the first time in 1896 by Sevioku Honda, Professor of Forestry at Tokio, accompanied by various people, amongst whom were a geologist and a topographer. After landing at Kelung, the explorers gained Ling-ki-ho, the last Chinese village, thenceforward they met with only the

aboriginal population. After the luxuriant cultivation of the low country, the travellers traversed a very wooded region, in which fig-trees, camphors, palms and tree-ferns predominated. The camphor-trees are particularly magnificent there, but already they are being destroyed by the dealers. Higher up, *Chamaecyparis* and *Cryptomeria* predominate, and yet higher are other conifers. No part of the ascent appears to be difficult; the slopes are not abrupt, and the soil is covered with trees or verdure to within a short distance of the summit, which is at an altitude of 4305 mètres (14,000 feet). The mountain is not volcanic; it is composed of slate and quartz. There was no snow, the time of the ascent being autumn. The aboriginal population is agricultural, and affords an interesting study.

The Gipsy Moth

The latest bulletin (No. 11 N.S.) which we have received from the Entomological Division of the U.S.A. Department of Agriculture, deals with the history of the 'Gipsy' Moth (*Porthetria dispar*) in North America. Mr L. O. Howard tells us that the insect was introduced in 1869 at Medford by Prof. Trouvelot, who wished to experiment with various European silk-spinning caterpillars. Some of his 'Gipsy' caterpillars escaped out of the window and established themselves in a neighbouring wood. After twelve or fifteen years' struggle with a hard climate, insectivorous birds, and occasional fires, the moths began to multiply and to spread about the surrounding parts of Massachusetts. In 1889 the caterpillars became a perfect plague at Medford, stripping trees completely of their leaves, falling about the road in thousands, invading houses, and getting into the food and the beds. During recent years the species has spread through a great part of eastern Massachusetts, but stringent efforts have been adopted to keep it in check, and it is hoped that the undesirable immigrant will ultimately be altogether exterminated.

The excessive multiplication of this moth in its new country will be of interest to British entomologists. Always with us a local insect, it is now probably extinct as a wild species in England though it is still kept up by moth-breeders in a 'domesticated' state.

The Muscle Scars of Fossil Cephalopod Shells

A paper of much interest and importance was read before the Linnean Society at their meeting on 3rd February last, "On the Muscular attachment of the Animal to its Shell in some Fossil Cephalopoda (Ammonoidea)," by G. C. Crick, of the British Museum (Natural

History). Having first briefly noticed previous descriptions and figures of what was believed to be the impression of the muscular attachment of the Ammonoid animal to its shell, the author pointed out the form and position of the 'shell-muscles' and of the 'annulus' in the recent *Nautilus*, and indicated the form of the impression of these structures as seen upon an artificial internal cast of the body-chamber for comparison with the fossil forms, in nearly all of which any indication of the muscular attachment there may be is preserved upon the natural internal cast of the body-chamber. After describing the character of the 'muscular scars' in an example of *Crioceras*, in which they were both very perfectly preserved, and the position of the 'annulus' as clearly shown in an Ammonite from the Oxford Clay, Mr Crick pointed out in a series of diagrams the impression of these structures in the various forms assumed by the Ammonoids, viz. — *Baculites*, *Hamites*, *Ancyloceras*, *Crioceras*, *Macroscaphites*, *Scaphites*, *Turrilites*, and *Heteroceras*, and several Ammonites, as well in *Clymenia*, and in some of the Goniatites.

The scars of the muscles are to be found in almost every instance on the inner or dorsal side of the body-chamber, near to the last septum, and are generally oval patches with the longer axis directed either antero-posteriorly or transversely; in spiral forms like *Turrilites* or *Heteroceras* they are curved with the shell, the one nearer the inner side of the coil being in either case the shorter. The annulus runs from one to the other of these scars round by the ventral side, not far from the septum, and in those forms where the latter is much folded it dips slightly with each lobe between the saddles. There is also evidence of muscular attachment to the shell between the two big scars and on the dorsal sides, so that the animal was connected with the shell all round the chamber. This, Mr Crick is inclined to infer, is evidence that the chambers of the whorls were filled with gas and not with water, since this complete attachment would be unnecessary in the latter case.

Mr Crick's researches effectually dispose of the notion, which was once held by many, that the animal was, so to speak, a tenant at will of its shell, like *Argonauta*, and show it to have been a tenant for life, like the Nautiloidea; but further in the table of affinities we cannot yet go.

Science in Lincolnshire

A NOTE and comment in our February number announcing the formation of a Lincolnshire Science Society has brought us a protest from the Lincolnshire Naturalists' Union, which includes among its officers some eminent men, some well-known naturalists, and some

whom we are glad to recognise as our own contributors. The Union has, we understand, existed for some time and publishes an account of its work and of the investigations of its members in the *Naturalist* and in *Lincolnshire Notes and Queries*. This Society also is advocating the establishment of a Museum in Lincoln, or rather the acquisition of a better building, in which to keep the collections which have already been brought together. It has therefore the same objects as the Lincolnshire Science Society. There is no doubt an unfortunate distinction between 'Science' and 'Natural History,' but it does not seem to us that there should be room in the county of Lincoln for two Societies intended to cover almost the same ground. We hope that some way will be found to fuse the energy of the upstart Science Society with the respectability of the older Naturalists' Union.

Feathers and Females

We are very glad to see, from various articles in the daily press, in *Nature Notes*, and elsewhere, that the crusade against the wearing of feathers by the harder-hearted sex is continued with vigour. We only regret that the necessity for this crusade is as great as ever it was. We know very well (what many of the crusaders seem to forget) that these feather-headed ladies read neither *Natural Science*, nor *Nature Notes*, nor even Sir William Flower's articles in the *Times*. As before, we can only appeal to our readers to do all that is in their power to influence those with whom they are naturally brought in contact. The apparent cruelty is probably due in most cases to mere ignorance, although we must confess that it does not seem to us that the woman who can commit the vulgarity of wearing the unnaturally dyed feathers that have been fashionable of late, would care how much suffering she caused to satisfy her unnatural and ridiculous taste.

A Card-Index to Current Zoological Literature

The Concilium Bibliographicum of Zürich-Oberstrass is now prepared to furnish sets of cards including references to articles on any zoological topic the subscriber likes to suggest. One can now order references to all papers published throughout the year on such subjects as the reptiles of Celebes, the salivary glands of molluscs, the instincts of bees, or the teleostean lake fauna of Westmoreland. Charges are now made according to the number of cards sent—the larger the number of cards the smaller the relative price. Cards are distributed every month, and if no papers on the given subject appear, a printed statement to that effect is sent to the subscriber.

The prices are undoubtedly cheap; even if one's subject necessitates less than 100 references *per annum*, the cards cost only 5 centimes apiece. 3000 references can be had for 39 francs. This reduction in price is rendered possible by the office having established its own printing press, and this will, it is hoped, enable the cards to be sent out with greater punctuality than has hitherto been possible. It is exceedingly unfortunate that the skilled typographer who was engaged should have been confined to the hospital for three weeks; but even this did not prevent the energetic Dr Field and his assistants from issuing no less than 200,000 cards from the press during December last. Some day it is possible that the international committee, convoked by the Royal Society, may provide zoologists with a complete and up-to-date subject catalogue; but even then, so far as we are acquainted with the plans that have been put forward, they do not propose to enter into one-hundredth part of the detail that is already possible to Dr Field. For the present, therefore, zoologists may be warmly recommended to consult their own advantage, and to assist a practical and useful work by subscribing to the Concilium Bibliographicum on the principle of 'no results, no pay.'

British Naturalists

We have received from Mr L. Upcott Gill the "Naturalists' Directory" for 1898. We are glad to see that a few more naturalists whose names are known to us have found their way into this issue, and since a complete work of this character would undoubtedly be of great service, we hope that in future Mr Gill will be even more successful in drawing up his lists. We notice his statement that *Natural Science* can be obtained post free for twelve shillings *per annum*. This, as we remarked last year, is not the case. A feature that might be made very useful is the list of works on natural science published during the previous year in the British Isles. This is said to be complete, but we have failed to find several works by well-known British authors which were reviewed in our own pages last year. There is also a list of Societies, Field-Clubs, Museums, etc., but this, like the other section, seems to us very incomplete. In short, the "Naturalists' Directory" deserves the support of our readers not for what it is, but for what it might be.

For Bibliographers

Those who have used Dr H. Carrington Bolton's "Catalogue of Scientific and Technical Periodicals" will be glad to hear that a new edition is soon to be published by the Smithsonian Institu-

tion. It is not surprising, considering the growth of science, that it has been found necessary to add 3500 titles since the first edition published in 1885. Another publication, which should be found useful to others than those for whom it is primarily intended, is the "International Exchange List" of the Smithsonian Institution, being a list of 9414 learned societies, museums, universities, and other bodies with which American publications are exchanged.

I

The Study of Variations

THE work of any generation must be largely influenced by current accepted ideas, which form, as it were, a mental environment, with which all the work of that generation will be coloured, and it follows that there must be considerable danger resulting from reactionary tendencies which frequently arise when the preceding generation has carried any given method of research beyond warrantable limits. If these current ideas do not happen to be true, errors, which may take years to eradicate, may arise, owing to theories being built upon these faulty conceptions. Harmful as this must be, it is, I believe, far less so than when a definite feeling or prejudice is formed in the mind against some method which had been previously misused.

For any prejudice thus formed will necessarily influence all work produced by all the minds so affected, and, further, while it is possible conclusively to disprove any erroneous idea, it is extremely difficult to remove any prejudice of the mind, when once formed.

In the present age it will, I believe, be found on reflection that the tendency, in all branches of science, is to neglect all purely theoretical conceptions, however sound, and to rely exclusively on practical deductions, for the most part directly deduced from experiment. This contempt for theory is, to my thinking, one of the most unsatisfactory elements in modern science, and has probably been directly induced by the extremely speculative and untrustworthy theories of the beginning of this and the latter part of the last century.

A theory of some sort is necessary to enable any investigator to collect facts or to perform experiments to any purpose, for facts or experiments, however numerous, are useless if any important factor is not accounted for, hence increasingly definite conceptions or theories always precede increasingly trustworthy investigation.

"It is a common error," says Poulton,[1] "to suppose that the intellectual powers, which make the poet or the historian, are essentially different from those which make the man of science. Powers of observation, however acute, could never make a scientific

[1] "Charles Darwin and the Theory of Natural Selection."

discoverer ; for discovery requires the creative effort of the imagination. The scientific man does not stumble upon new facts or conclusions by accident; he finds what he looks for. The problem before him is essentially similar to that of the historian who tries to create an accurate and complete picture of an epoch out of scattered records of contemporary impressions more or less true, and none wholly true. Fertility of the imagination is absolutely essential for that step, from the less to the more perfectly known, which we call discovery.

" But fertility of imagination alone is insufficient for the highest achievement in poetry, history, or science ; for in all these subjects the strictest self-criticism and the soundest judgment are necessary in order to insure that the results are an advance in the direction of truth."

This passage appears to me to be singularly applicable to evolutionists of the present day. If an examination of the facts, collected by evolutionists with reference to heredity, be made, it will be found that in most cases elimination of doubtful factors has not been attempted, because it has been assumed that variations must be adaptive or non-adaptive in character, and that consequently the case for or against use-inheritance will be decided by thus directly appealing to variations as they exist in nature rather than to specially prepared test cases. The aim of this paper is to endeavour to demonstrate that this question cannot be so settled, even if it were always possible to know whether a given variation were adaptive or otherwise. And when it is considered how difficult it is and what prolonged study is required to assert whether any small variation in man is useful or otherwise, the difficulty of arriving at a similar conclusion in any other sub-order or species, which of necessity is much less thoroughly studied, must be immense.

If a statement of the views held by different evolutionists be compared with objections raised to these views, the inconclusiveness of the objections will be noticed in most if not all the cases so examined.

If the opinions of Cope, Henslow, Darwin, or Weismann on this subject be shortly stated, the difficulty of arriving at a satisfactory conclusion will be rendered still more obvious, while the intermediate positions held by other evolutionists, such as Spencer, Huxley, Romanes, Galton, make the present position peculiarly unsatisfactory for future work in evolution until this question is settled.

Darwin.—" As far as I am able to judge, after long attending to the subject, the conditions of life appear to act in two ways,—directly on the whole organism or on certain parts alone, and indirectly by affecting the reproductive system. With respect

to the direct action, we must bear in mind, as Professor Weismann has lately insisted, and as I have incidentally shown in my work on 'Variation under Domestication,' there are two factors—namely, the nature of the organism and the nature of the conditions. The former seems to be much the more important; for nearly similar variations sometimes arise under, as far as we can judge, dissimilar conditions; and, on the other hand, dissimilar variations arise under conditions which appear to be nearly uniform. The effects on the offspring are either definite or indefinite. They may be considered as definite when all or nearly all the offspring of individuals, exposed to certain conditions during several generations, are modified in the same manner" (p. 6 "Origin of Species," 6th edition, 1884).

"Indefinite variability is a much more common result of changed conditions than definite variability, and has probably played a more important part in the formation of our domestic races" (p. 6 "Origin of Species").

Weismann.—"The cause of hereditary variation must be due to the direct effect of external influences on the biophors and determinants" (p. 415, Weismann, "Germ Plasm: A Theory of Heredity").

"We can none the less avoid assuming that the elements of the germ-plasm—*i.e.*, the biophors and determinants—are subject to continual changes of composition during their almost uninterrupted growth, and that these very minute fluctuations which are imperceptible to us are the primary cause of the greater deviations in the determinants, which we finally observe in the form of individual variations" (p. 417, "Germ Plasm").

"Of course, I see no reason for assuming two kinds of hereditary variations different in origin. Still, it is likely that only a relatively small proportion of the numberless individual variations lie on the path of phyletic advancement, and so under the guidance of germinal selection mark out the way of further development; and hence it would be quite possible in this sense to distinguish definitely directed individual variations from such as fluctuate hither and thither with no uniformity in the course of generations. The root of two is, of course, the same, and they admit of being distinguished from each other only by their success, phyletic modification, or by their failure" (Note, p. 17, "Germinal Selection").

Henslow.—"I would describe the process, therefore, once more as the result of the responsive power of protoplasm, on the one hand, and the forces of the external environment on the other.

These two factors I take to be amply sufficient for the whole of the evolution of plant structures, without any aid from Natural Selection whatever" (p. 32, "Origin of Plant Structures").

Cope.—"Many of the zoologists of this country, in common with many of those of other nations, have found reason for believing that the factors of evolution which were first clearly formulated by Lamarck are really such. This view is taken in the following pages" ("Primary Factors of Organic Evolution." E. D. Cope).

To summarise these various positions:—

Henslow considers that the sole factors in the formation of species are the direct action of the environment and the responsive power of protoplasm, and that Natural Selection is of use only in so far as it is able to select the vigorous, solely on account of their being so, but has no power to select and develop any particular portion of an organism separately.

Cope, who, while largely agreeing with Henslow's position, considers Natural Selection a subsidiary factor in the formation of species.

Darwin, who considered Natural Selection to be the main factor in developing species, but considered variations as due largely to the direct action of environment, Natural Selection subsequently developing variations so produced, also considered that changed environment could modify directly the reproductive system and hence influence heredity.

Weismann, who believes that use-inheritance plays no part in species formation, and extends the action of Natural Selection to the biophors, etc. Natural Selection and indefinite variability are therefore the only factors in evolution.

In criticising the position of the Neo-Lamarckians, I shall mainly base my arguments on Professor Henslow's position, because he has taken the most definite, and at the same time the most extreme, position. I do not wish to be understood to doubt the facts adduced, or even to assert that the conclusions drawn are incorrect, but simply to show that the facts as stated, and the arguments drawn from these facts, do not on logical grounds prove the position taken up.

I shall choose for the statement of his views the epitome given in the preface to "Origin of Plant Structures," because it is the most condensed statement of his position, and appears to accord fully with an article contributed to this journal at a later date (September 1897).

"I. Darwin asserts that Natural Selection has no relation whatever to the primary cause of any modification of structure.

"II. A changed environment—especially that of cultivation—stimulates variability—*i.e.*, the innate capacity of varying, which results in variations of structure. This fact is recognised by Darwin, Weismann, Spencer, and all other biologists.

"III. Under cultivation variations, especially after several years, are often indefinite, as may be seen in wheat, maize, and in numerous garden plants (but not in all, as sea-kale and asparagus). Hence artificial selection is absolutely necessary.

"IV. In nature variations are always definite, and not exceptionally so, as Darwin thought. The consequence is that 'all or nearly all individuals become modified in the same way.'

"V. The result of the preceding is that a new variety, and thence a new species, would be produced 'without the aid of Natural Selection.'"

I propose to deal with the last conclusion first; the statement is made that if definite variations occur "a new species would be produced 'without the aid of Natural Selection.'"

And that this meaning should be perfectly clear, and that the total exclusion of Natural Selection in the formation of species is intended, he elsewhere places the words "a seedling survives solely because it is vigorous" in italics.

If for the sake of argument it is granted "that variations are always definite," and also that definite variations are unexplainable by Natural Selection, and therefore that "all or nearly all the individuals become modified in the same way," it is still necessary to show not merely that all are similarly modified, but also that they are all equally thoroughly so, otherwise the variation that is most adapted will probably or at least possibly be selected, and Natural Selection will thus become a factor of some importance.

Again, it is stated that "In nature, variations are always definite," and it apparently is assumed by Henslow and other Lamarckians that this of necessity upsets the Darwinian position.

It does not appear to me seriously to weaken the theory of Natural Selection, because more or less direct variations would ultimately be produced by this factor alone.

Assume that at any period, however remote, variations were completely indefinite, and suppose that an individual A gave rise to 11 varieties, a^1 a^2 a^3 up to a^{11}; assuming that the odd numbers were more or less unadaptable varieties, 2, 4, 6, 8 and 10 would be selected, and then on the simple assumption of hereditary transmission of tendencies, which all biologists admit, these varieties 2 to 10 would give rise to still more adaptable varieties, until each

variety was definite, only in varying degree, the more or less unadaptable being increasingly rigorously weeded out in each succeeding generation, and this would continue to be so as long as only a very gradual migration or no migration at all recurred, provided conditions remained more or less the same. Directly, however, any sudden change occurred, the balance of adaptable variations would have to change, and indefinite variations would become once more apparent. Now this change might perfectly explain the fact that variations are more definite in nature than under domestication, because changes of environment are less extreme in nature than when under the selective power of man, also this very change in the balance of variations might itself be the cause of atavism, etc., which has frequently been noticed under some of these extreme changes in environment or when crosses between allied species have occurred.

Thus, so far from definite variations being a difficulty in the acceptance of the theory of Natural Selection, it would be precisely what on *a priori* grounds would be expected.

Finally, there are those classes of variations which are said to be directly modified in response to environment and in which the variations have possibly been inherited after the third or fourth generation of exposure to the modifying cause. To cite one instance, the fact of "chlorophyllous tissue being much more developed in sunlight than in shade."[1]

It is not enough to show that sunlight causes an increased amount of chlorophyll to be formed, for the fact that increased function brings about a compensatory increase in the tissue so exercised is admitted by all biologists: it is not enough even to demonstrate that this increase is detectable when the variety thus formed is again bred under the normal amount of sunlight. It must further be shown: (1) How many seeds or cuttings of the particular plant were set; (2) How many of those that were set died; (3) What was the result of a control experiment in which the same conditions were observed in everything but the excessive light; (4) That in all the experiments the cuttings were so situated that the growth of each was, as far as possible, entirely free from interference by the others; (5) That the chlorophyll does not develop more quickly, when, with otherwise similar conditions, rigorous Natural Selection is allowed to occur; (6) It must also be shown, as far as possible, that no immediate ancestor of the selected variety experimented upon has had a greater supply of chlorophyll, and consequently that the action that the sunlight had apparently induced was not in reality only a favourable reversion.

In short, to disprove the action of Natural Selection, it is neces-

[1] P. 72, "Origin of Plant Structures."

sary to show something further than that variations are definite, or even that direct adaptive modification may occur which can subsequently become hereditary. Natural Selection must, as far as possible, be totally excluded, reversion and influence of climate on reproductive organs also more or less satisfactorily eliminated, before anything like a test case can be adduced. This case must still further demonstrate, if it is to disprove anything more than the Weismannian position, the uselessness of Natural Selection by separate control tests.

The same arguments will apply to Cope and other Neo-Lamarckians, although less forcibly, on account of Natural Selection being considered a subsidiary factor.

But the position is if anything even more difficult for the Neo-Darwinians, as it would be necessary to produce definite hereditary variations by Natural Selection without any change in environment, and the elimination of such a wide series of factors would be exceedingly difficult to accomplish. The extreme difficulty of this position has been, to my thinking, so conclusively proved by Romanes and Spencer, that the only means of obtaining definite results will be by a complete revision of the methods used.

My object has been to show that equally plausible contrary positions may be taken up from the facts adduced by either side.

If this is so, it would seem probable that some fundamental mistake must be responsible for this curious position, and the cause, I believe, exists in the extremely uncertain meaning given to such terms as adaptive, definite, and indefinite variations. A more rigid and exclusive use of these terms might, by rendering the difficulties more apparent, help them to be more easily overcome.

Some such classification as the following might be resorted to:—

Unadaptive Variations.—Having no selective value on account of all the variations being wholly unfitted for their environment.

Indefinite Variations.—Capable of selection, but varying in all directions round a central position.

Definite Variations.—All variations being of selective value, but that value varying in degree.

Adaptive Variations.—All equally modified for special conditions, and therefore incapable of being selected except from general strength or vitality.

These divisions can only be approximately correct, but would serve to limit the meaning of these terms and aid the formation of clearer ideas on this subject.

Lastly, some classification of possible origin of variations which might be applied equally to both animal and vegetable life, by

which common comparisons might be estimated, is, I think, essential to a better understanding of the subject.

Macalister[1] divides the varieties of human organism into nine classes, as follows: (1) Hetero-meral or anomalies of quantity; (2) Metaplastic or anomalies of material; (3) Didymal, anomalies of repetition; (4) Diastematic, anomalies of cohesion; (5) Antithetic or anomalies of alternation; (6) Metathetic, anomalies of position; (7) Homoiotic, anomalies of series; (8) Atavistic, anomalies of inheritance; (9) Neoplastic, anomalies of new formation. Such a classification, perhaps with slight alterations, might be universally applicable. It has an advantage over most others on account of each term having a clearly defined meaning. But whether this or some other classification be adopted, such as Osborn's, it should at least be universal in its adoption, and should cover all the chief divergences from any common average taken as standard.

In conclusion, I fail to see the use of continuing this discussion as it now stands. No case has been yet adduced by one side which has in any sense been regarded by the other as final. If I am right, the cause is to be sought in the loose way in which variations have been classed. If it is urged that to make such a close study would require almost a lifetime, and exceptional capabilities besides, it should be recollected that it would at least settle a question of extreme difficulty, and one which, until answered, will greatly interfere with the progress of biology, while its solution would in addition probably help to solve many problems in modern medicine and kindred sciences which must otherwise remain more or less incomprehensible.

J. LIONEL TAYLER.

[1] Third Boyle Lecture, Oxford.

II

The Late Lamented Latreille

A Study in Names

PIERRE ANDRÉ LATREILLE died between sixty and seventy years ago, and now some amiable persons in Washington wish to canonise him, not for any of his good works, but for one of his weakest. In 1810 he published a volume, neither his first nor his last, on the inexhaustible theme of crustaceans, arachnids, and insects. In it he still mixes up the isopods with the arachnids, although Lamarck had nine years earlier properly placed them among the crustaceans. In it he passes some not undeserved commendations on his own earlier writings, and on those of J. C. Fabricius some criticisms of questionable temper and taste. After a tolerably interesting introduction of some eighty pages, he devotes the bulk of the volume to a methodical table of genera, containing nothing, or next to nothing, that was new. He leaves his readers to guess at what dates the genera had appeared, and by what authorities all of them except his own had been established. He leaves his readers also to guess whether any particular genus contained but a single species or a score. In the last twenty-four pages, in small print, as though an after-thought or as a substitute for an index, he gives a "Table des genres avec l'indication de l' espèce qui leur sert de type." This precious performance is now being elevated into a standard of scientific nomenclature, not because of any imagined thoroughness or excellence, but simply because of its imperfection. For each genus, whether large or small, it mentions one species and only one, with no word of description, with no reason given for the selection, and this casual mention in a college manual is supposed, forsooth, to confer the rights of a type-species!

It becomes important, therefore, to cast a little light upon Latreille's catalogue, and to ascertain the meaning and application of the word 'type' which he uses at the head of it. But a glance must first be taken at more modern writers. In 1896-97 a lively discussion was carried on in *The Annals and Magazine of Natural History*, ending, as I fondly hoped, in acceptance of the decision that the lobster was properly assigned to the genus *Astacus*. The controversy had been opened somewhat earlier in Dr Herrick's

book, "The American Lobster," and the review of that book which appeared in *Natural Science* for June, 1896. Dr Herrick's advisers on the subject were Dr Walter Faxon and Miss Mary J. Rathbun. They have since returned to the attack.[1]

Dr Walter Faxon is, I believe, the greatest living authority on crayfishes. Miss M. J. Rathbun is one of our foremost authorities on crabs. Both these writers are distinguished for their learning, industry, and acuteness. To see them both relying on Latreille's feeble book is like contemplating the first Napoleon as Emperor of Elba. In the outset these eminent naturalists were confessedly under the impression that, in writing my "History of Crustacea" (Internat. Sci. Series, 1893), I had overlooked Latreille's "Considérations Générales" of 1810, whereas, in point of fact, I had given them my particular consideration and had, then as now, come to the conclusion that on questions of nomenclature, the book containing them was of no importance.

It will probably be admitted on all sides that when an author establishes a genus for a single species, that species must be regarded as the type of the genus. It will probably be further admitted that, of several species contemporaneously placed in a genus, any one singled out by the author himself as the type of the new genus holds a preferential right to the generic name. But, supposing that the author has not selected a type, and that the species of his genus have eventually to be re-distributed among several genera, which of them is then entitled to be retained in the original genus? Apart from other guidance, might not one innocently suppose that the species placed first by the author ought to have precedence? It has priority, even if it be on the same page with its successors. I ask whether one might not innocently suppose this, because the innocence of the supposition is called in question by Dr Faxon. He says:—

"It is hard to believe that this contention of Mr Stebbing's is made in good faith, involving as it does an unreasonable and long-discarded method of ascertaining a type. Such a method is repudiated every time we concede to an author, who first sub-divides a genus in which no type has been specified, the right to restrict the original name to such part of it as he pleases. It is not true that the first species is presumably the author's implied type. Fabricius's genus *Astacus* was formed by a dismemberment of the genus *Cancer* of Linnaeus, and the sequence of the two species under

[1] A revision of the nomenclature of the Brachyura, by Mary J. Rathbun. *Proc. Biol. Soc., Washington*, vol. ii. pp. 153-167. 1897.

List of the Decapod Crustacea of Jamaica, by Mary J. Rathbun. *Annals of the Institute of Jamaica*, vol. i., No. 1. 1897.

Observations on the Astacidae in the United States National Museum and in the Museum of Comparative Zoology, with descriptions of new species, by Walter Faxon. *Proc. U.S. Nat. Mus.*, vol. xx., No. 1136. 1898.

consideration [European lobster and crayfish] in Fabricius's works was undoubtedly derived from the 'Systema Naturae,' wherein (in the twelfth edition) *Cancer gammarus* stands as No. 62, *Cancer astacus* as No. 63, in the genus *Cancer*. A better, though not a valid, claim might be set up for *A. fluviatilis* as Fabricius's implied type of his genus *Astacus*, since that species is the *Cancer astacus* of Linnaeus."

On such a subject the imputation of bad faith can only make one fancy that he who imputes it has been listening to the old machiavellian advice, "You have no case; abuse the plaintiff's attorney." Between scientific comrades such innuendoes should surely be dispensed with, and they are almost always a tactical mistake, since retaliation is often only too tempting and too easy. But the whole passage seems founded on misconceptions. "It is not true," Dr Faxon says, "that the first species is presumably the author's implied type." There is really no question at all about what is implied or what is to be presumed, but only about the rule convenient to follow, when an author has himself left us in the dark. In that case to follow the rule of priority is not merely not unreasonable, but truly the simplest and most consistent plan that could be adopted. It is an astonishing argument to urge that Fabricius could not have intended the lobster to be the type of his genus *Astacus*, because he followed Linnaeus in giving it precedence. It is, besides, a plain matter-of-fact, that in arranging the species of this genus, Fabricius by no means slavishly follows the Linnean order. But that point need not be laboured, since Dr Faxon takes his stand on a rule not affected by it, a rule laid down long after the time of Fabricius and Latreille. According to this rule or recommendation, "When the evidence as to the original type of a genus is not perfectly clear and indisputable, then the person who first sub-divides the genus may affix the original name to any portion of it at his discretion, and no later author has a right to transfer that name to any other part of the original genus." Whether wisely or not, that rule gives a special privilege of ignoring strict priority to persons who deal with what may sometimes be a very difficult and troublesome task. We have now to consider the retrospective action of that rule upon the question before us.

It is not disputed that until 1819 the genus *Astacus* contained both the European lobster and the European crayfish. Then Leach assigned the latter to a new genus, *Potamobius*, that is to say, he first subdivided the genus *Astacus* as it then stood, and he affixed the original name to the lobster. You may say he followed the rule of strict priority. You may say he did what he did at his discretion. To persons of commonplace intellect like myself it must seem as if, either way, his names were bound to stand. The illus-

trious American professor Dana rejects them on grounds which, I think, Dr Faxon and Miss Rathbun cannot possibly accept; but, apart from those grounds, Dana declares that "Leach has undoubted priority."

Now, then, at last we may return to Latreille and his magic volume of 1810. Latreille was not the author of the genus *Astacus*, and he made no attempt to subdivide it, so that it is difficult to see by what rule he could have any right to assign the type. But it is superfluous to debate his right, if he never did or thought of doing that which Dr Faxon so confidently affirms him to have done. Observe, first, that the family to which Latreille assigns *Astacus* as the initial genus, is called by him "Homardiens, Astacini," and homard is not French for crayfish, but for lobster. Yes, but then in that table of genera "avec l'indication de l'espèce qui leur sert du type," he gives "Ecrevisse, *Astacus fluviatilis*, Fab.," and to anyone who urges that Latreille was only giving an example of the genus, not the type in any technical sense, Dr Faxon makes reply:—

"As I understand it, the French word 'type' means 'model,' 'type,' or 'standard,' not 'example' or 'illustration' (Gallicè *exemple*). I see no reason for going behind Latreille's plain words to indulge in uncertain speculation concerning his possible meaning. If Mr Stebbing is unwilling to allow Latreille the use of the word 'type' in its technical sense, by what 'statute of limitation' will he fix the year when the word acquired that meaning? Even if it be admitted that there is some doubt concerning the significance of the word 'type' as employed by Latreille, the benefit of the doubt should, by a reasonable ruling applicable to all such cases, be given to a long established terminology."

"Some doubt," indeed! Happily in this instance there is no doubt. In spite of his great learning, Dr Faxon seems to speak of Latreille as if he were a man of only one book. Latreille was a voluminous writer. He can throw abundance of light upon himself. In 1825 he published his "Familles naturelles du Règne Animal," in which genera are mentioned for the most part only under French names, and in which scarcely any species are mentioned at all. But of the very few which happen to be noticed one is *Astacus marinus*, the lobster. Now, seeing that in this work Latreille recognises three distinct genera, "Nephrops, Homard, Ecrevisse" (p. 279), and that he calls the homard *Astacus* (p. 274), he must have accepted a different generic name for the ecrevisse. He evidently knows nothing about his own fine doings. It would have amused him to learn that he was no longer at liberty to speak of the lobster as *Astacus*, because already in 1810, in a happy-go-lucky list of species, the river crayfish had been "speci-

fied as the type" of that genus—by himself. But light can be shed on the subject from his earlier works as well as his later. In 1802 he published the first four volumes of his "Histoire naturelle, générale et particulière, des Crustacés et des Insectes." Here, just as in 1810, we have "Famille cinquième. Homardiens; *astacini*," with the first genus "Ecrevisse; *astacus*," signalised by a single species thus—"*Exemple. Astacus fluviatilis*, Fab." A little research, therefore, would have shown Dr Faxon that Latreille uses 'exemple' and 'type' as precise equivalents. The sixth volume of the "Histoire," published in 1803, describes seven species of *Astacus*, giving the first place to "Ecrevisse homard; *astacus marinus*." In the "Genera Crustaceorum et Insectorum," published in 1806, where only two species of *Astacus* are mentioned, it is still the *Astacus marinus* that holds the place of honour. But if this local position on the printed page gives no indication of Latreille's opinion as to the typical species, why, it may be wondered, does he trouble himself to tell us in the "Histoire" (vol. vi., p. 230) that, "il est facile de conclure des observations d'Aristote, que son *astacos* est l'écrevisse de mer ou le homard," and offer proofs that by this name "Aristote désigne positivement le homard." It cannot be supposed that he wished to flout Aristotle by taking as type of the genus, not the lobster which Aristotle positively designates, but the river crayfish, of which, he says, Aristotle makes no mention.

The same line of argument which indicates for Leach the generic names of the lobster and the crayfish, will indicate for Stimpson the distinction between the prawns *Leander* and *Palaemon*, and once more for Leach the distinction between the sand-hopper *Talitrus* and *Orchestia* the shore-hopper. *Talitrus* is Latreille's own genus. In 1802 he gave two examples of it. The first has from then till now retained the name of *Talitrus*. The second was transferred to *Orchestia* by Leach in 1814. It is this latter species which Latreille in 1810 names by itself as 'type' of the genus *Talitrus*. But that he had the least intention of establishing it as the type, it is not only hard but impossible to believe, for as well in the "Nouveau Dictionnaire d'Histoire Naturelle," vol. i., 1816, as in the "Familles Naturelles" of 1825, and "Le Règne Animal," vol. iv., 1829, he recognises *Orchestie* and *Talitre* as distinct genera. Now *Orchestia* could only be a synonym of *Talitrus*, were the species mentioned by Latreille in 1810 as an illustrative example to be taken as a governing type.

In 1801 the brilliant Lamarck published his "Systême des Animaux sans Vertèbres," a manual on much the same plan as Latreille's volume of 1810. In his "Avertissement" he says: "Pour faire connoître d'une manière certaine les genres dont je donne ici les caractères, j'ai cité sous chacun d'eux une espèce

connue, ou très-rarement plusieurs, et j'y ai joint quelques synonymes que je puis certifier; cela suffit pour me faire entendre." The expressions, 'j'ai cité une espèce connue' and 'cela suffit pour me faire entendre,' are exactly applicable to Latreille's list in 1810, which, whatever its merits or shortcomings, must be absolutely acquitted of any charge of type-mongering ambition.

Dr Faxon, while upholding *Astacus* as a genus of crayfishes, divides it into two subgenera, *Cambaroides* and *Astacus*, and, as he identifies the *Astacus fluviatilis* auctorum with *Cancer astacus* Linn., the name of this species with him will become *Astacus* (*Astacus*) *astacus* (Linn.) Faxon. It is a matter of taste, but one would scarcely expect a countryman of Frank Stockton, Mark Twain, Artemus Ward, and so many others capable of acutely distinguishing between the serious and the absurd, to struggle for the honour of being stepfather to such a designation. By ignoring the rightful claims of *Potamobius* Dr Faxon is only prolonging confusion in a group with which his own reputation is so honourably connected.

THOMAS R. R. STEBBING.

III

Pentacrinus: a Name and its History

IT is the misfortune of Systematic Zoology that it combines two very different studies: the study of things and the study of names. The former is the more important; but the latter is necessary for the clear interchange of ideas. In the study of things it may be useful to examine the writings of those who have gone before; but, except for the sake of giving honour where honour is due, or of enlivening matters by cheerful criticism, such examination is not forced upon us: it may even lead us astray. The study of names, however, cannot but be a study of other men's opinions as transmitted to us in their writings. The former study—of things or facts—is bound by no laws save those of nature and truth; the latter is circumscribed in every direction by the rules established by zoologists themselves. It is no wonder if the arbitrary, man-made branch of the science occasionally accords but ill with what we believe to be the facts of nature.

The rules of nomenclature are rigid, but knowledge changes, and, we trust, advances. Hence a name that was proposed with a definite connotation ceases to have that connotation, and may come to mean almost the opposite of what its author intended. We systematists know better than anyone else that these things are absurd, but no better method of naming has ever been proposed. Were we to make a clean sweep of all names and to start with a *tabula rasa*, the strictest adherence to the strictest code of nomenclature could not prevent a recurrence of confusion. The sole remedy is omniscience. Admitting all this, it is none the less, but rather the more, our duty to protest against those who, under the seductive guise of expediency, attempt to override or to evade the accepted rules. Authority is a shadow, customs change, "our little systems have their day," and so forth. None of these is an excuse for proven error.

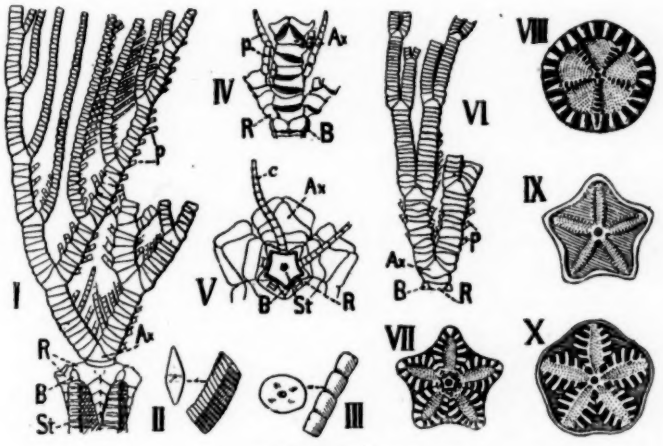
These homilectics are intended to pave the way for a protest in one particular instance: the misuse of the name *Pentacrinus*. Every naturalist thinks he knows what *Pentacrinus* is, but it is probable that a palaeontologist and a neontologist would point to very different forms as representative species; while it is certain that what zoologists as a rule understand by *Pentacrinus* has no right to that ancient title.

First let us see what are the concrete facts that have to be represented in nomenclature.

The Sub-family Pentacrininae contains crinoids that have a long stem (*St*), with five angles more or less marked, and with cirri (*c*) or side-branches springing from the sides in whorls, usually of five. The stem possibly ends in a root in early life; but it has the power of breaking away just below each whorl of cirri, so that the adult crinoid is to some extent free-moving, and attaches itself by the cirri at the end of its stem. The stem-ossicles or columnals are united by five bundles of ligament, on which they are threaded, as it were, and the articular surfaces are marked by crenelate elevations which surround these bundles, and so assume a petaloid shape; the elevations of one columnal fit into depressions in the next. This stem supports a crown; and this crown may be divided into (*a*) a cup containing the viscera, and (*b*) branching arms. The cup consists essentially of three circlets of five plates alternating with one another, namely: Infrabasals, next the stem; Basals (*B*); and Radials (*R*) supporting the arms. But as a rule the infrabasals are minute or atrophied in the adult, and the basals also are often so reduced in size that the radials rest partly on the top columnal; while to make up for this the lower ossicles of the arms (the proximal brachials) form part of the sides of the cup, *i.e.*, help to surround and support the viscera. The arms always bear fine branchlets, given off regularly on alternate sides, and known as pinnules (*p*), and in addition the arms always fork at least once.

The Sub-family includes at least four genera, which, to avoid controversy, I shall call A, B, C, and D. A begins in the Trias and persists to Eocene time. It has a cylindrical or prismatic stem, and the crenelations on the articular faces of the columnals are confined almost entirely to the circumference, while the five ligament areas are limited by five thin, raised and slightly crenelate lines, which radiate from the centre of the columnal (fig. VIII.). The cup is not well known; the arms fork more than once into equal divisions. B was dominant in Liassic seas, less abundant in Oolitic. The crenelations on the articular faces of the columnals are delicate, and are arranged in five narrow petals quite distinct from one another (fig. IX.). The cirri are often numerous and are laterally compressed so as to have an elliptical or rhomboid section (fig. II.). In the cup, infrabasals can usually be distinguished; the radials are prolonged downwards over the columnals next the cup in spine-like processes (fig. I.); they support large arms which branch in such a way that each main division bears a number of minor armlets all coming off on the same side (fig. I.). C is found in Triassic and Jurassic rocks, but became more prominent in Cretaceous times, and is the chief representative of the Subfamily at the present day. The crenelations of the columnals are

coarse, and are arranged in petals so broad that they run together towards the centre (figs. VII., X.) and are not distinct as in type B. The cirri are transversely elliptical or circular in section (fig. III.), and the whorls not so numerous. Infrabasals have only been detected once, and in that instance are quite minute. Basals may or may not form a complete circlet. The radials are not prolonged downwards into spines. The arms fork once or many times, but always equally



PENTACRININAE.

I. *Pentacrinus fossilis*, Blumenbach. Portion of stem, cup, and portion of arm showing rami, ramules, and pinnules.
II. *Pentacrinus fossilis*. Portion of a cirrus, and articular facet of a cirral.
III. *Isocrinus asteria*, Linn. sp. Portion of a cirrus, and articular facet of a cirral.
IV. *Metacrinus moseleyi*, P. H. Carpenter. Cup and proximal portion of an arm. After P. H. C.
V. *Isocrinus pendulus*, H. v. Meyer. The cup seen from below, with portion of stem, bearing cirri, still attached, and with proximal brachials. After H. v. Meyer, em P. de Loriol.
VI. *Isocrinus pendulus*. A radius, showing regular bifurcation of arm. After De Loriol.
VII. *Isocrinus amblyscalaris*, Thurmann. Closely allied to or identical with *I. pendulus*. Articular facet of an internodal columnal. After De Loriol.
VIII. *Balanocrinus subteres*. Münster, sp. Articular facet of an internodal, diagrammatised from E 991 in Brit. Mus. Our type A.
IX. *Pentacrinus fossilis*. Articular facet of an internodal, diagrammatised from 51170 in Brit. Mus. Our type B.
X. *Isocrinus asteria*. Articular facet of an internodal, diagrammatised from E 6456 in Brit. Mus. Our type C.

(fig. VI.). There are two brachials below the first forking (*Ax*), and these bear no pinnules. D is known only from recent seas, and occurs in the West Pacific. It resembles C, but has 5-8 brachials below the first arm-forking, and of these the 3rd, 4th, 6th and 7th bear pinnules (fig. IV.). The basals form a complete circlet.

No dispute arises with regard to the names of A and D. De Loriol has perhaps strained a point in extending to A the name of

Balanocrinus, primarily applied by L. Agassiz to an abnormal stem-fragment of another genus; but nothing would be gained by contesting his action. D is the genus called *Metacrinus* by P. H. Carpenter, who took the name from a MS. label of Wyville Thomson.

C and D are the types about which there is confusion. C is called *Extracrinus* by Carpenter (1884) and De Loriol (1886), while the name *Pentacrinus* is restricted by them to D. This course has been followed in the Geological Department of the British Museum. I shall now prove that by the rules of nomenclature, by the practice of many early naturalists, and by a literary history of three centuries, the name *Pentacrinus* is the property of type C.

First let us remember that living examples of a Pentacrinid have been known to men for less than a hundred and forty years, while the fossils have been well known for centuries. The name *Pentacrinus* itself is supposed to be due to Georg Bauer, better known as Agricola. This is what he says in his work "De Natura Fossilium" Lib. v. pp. 256-8 (1558). First he describes the "trochites," circular bodies with smooth circumference but with upper and lower surfaces covered with striae that radiate from the centre, with a glittering fracture and regular cleavage like Jew-stones [*i.e.*, fossil spines of sea-urchins], giving off gas when placed in vinegar. The "entrochos" consists of trochites not yet separated from one another. Next, under the name Ceraunia, Agricola describes certain bodies, which appear to be fossil sea-urchins. He then proceeds: "But as the Entrochos when divided represents the effigy of wheels, so the Encrinos does of lilies. For when one angular part is separated from another, each shows a set of five lilies. For the elevations of the one fit into the striae of the other. And any such part has five angles and as many sides, and on each is a quintet of lilies. Whence it may be called pentacrinos in Greek. But just as the entrochos consists of many trochites, so the encrinos consists of many pentacrini. . . . The stone of the pentacrini when broken has the colour, smoothness, and glitter of the Jew-stone."

It is clear from this that the syllables "crinos" originally referred solely to the leaf-like or petaloid markings on the joint-surfaces of the stem, and not to the lily-like appearance of the whole animal, just as the Japanese call a similar fossil 'the plum-flower stone.' Further, that both *Encrinos* and *Pentacrinos* applied to the same fossil under different aspects. This was understood by the successors of Agricola, among whom Lachmund (1669) is conspicuous. It was, however, this writer who introduced confusion, for in his chapter "De Encrino et Pentacrino" he says, "To Encrinus I refer a stone, which consists of many other small and angular stones, the elevations of which are inserted into the striae of another, just as the skin, *imprimis* in the anterior part,

around the legs of fowls, as is seen in the figure, where are two series of separate ash-coloured stones, and others whitish, elegantly joined into one mass; the substance is that of the Jew-stone." The figure represents five biserial arms of a crinoid, broken away at one end, so as to expose the pinnules. There has never been any doubt as to Lachmund's meaning; the genus to which he referred was soon figured by others from more complete specimens, and was distinguished as *Encrinus* or *Encrinos Lachmundi*. Under that name it was referred to by the illustrious Luidius or Lhuyd (1699). But it was soon discovered that the arms of *Encrinus Lachmundi* were attached to an *Entrochus*, or stem of round columnals with radiating striae, and not to an *Encrinus*, or stem composed of *Pentacrini*. Then there arose a false etymology, and many who did not know their literature supposed that *Encrinus* meant a fossil like a lily with its flower closed: "Κρίνον heisst eine Lilie, Ἔγκρινος soll daher eine geschlossene Lilie bedeuten," wrote J. S. Schroeter (1778), and laughed at those who, as Bertrand (1763) and Guettard (1761), were conservative enough to use *Encrinus* in what, as he seems to have forgotten, was its original sense. The idea seems to have arisen with Harenberg (1729) who supposed that *Encrinus Lachmundi* actually was a petrified lily; hence the term "stone-lilies" even now applied to the Crinoidea. There were not wanting writers to protest with a mild solemnity that this fossil was really much more like a cob of maize than any lily whether open or closed.

About the time of Linnaeus then there were two views or practices. The one applied *Encrinus* or *Encrinites* (the terms were used indifferently) to stems composed of *Pentacrini*, *i.e.* star-marked columnals, also known as *Asteriae*. The other, and the more favoured, distinguished *Encrinus* from *Pentacrinus* thus:—The Encrinite has a round stem or *Entrochus*, composed of *trochitae*, above this is the "Gelenkstein," called *Pentagonum* by Rosinus (1719), Lachmund (1669), and others, [= Cup], and this supports a crown which consists of rays. The Pentacrinite has a stem, usually pentagonal, the separate columnals of which are the so-called *Asteriae*, which form the *Asteriae columnares*; it has no "Gelenkstein," and its crown is fasciculate.

Linnaeus in the tenth edition of his "Systema" mentioned no recent stalked crinoid; while both here and in the later editions, fossil crinoids were placed in the mineral kingdom under the comprehensive and inappropriate heading *Helmintholithus*, with hardly an attempt to arrange them under the binominal system.

The earliest writer after 1758 to help us in this matter is C. F. Schulze (1760), whose work appears to Mr C. D. Sherborn and myself to contain Latin names used in the Linnaean manner, such as *Astropecten regularis*, *Palmipes coriacea*, and *Astrophyton arach-*

noides. This is the earliest post-Linnaean work to which I can trace the name *Encrinus*, here applied in the sense of Schroeter to the fossil that is now universally known by that name, though ascribed to Lamarck, who was forty years later. There is a full description with figures of the common species usually known as *E. liliiformis*, but no trivial name is used by Schulze. This step finally removes *Encrinus* from all rivalry with *Pentacrinus*.

Again the fact must be emphasised that up to this time the name Pentacrinus or Pentacrinite had of necessity been applied to some fossil crinoid. Further, I would draw attention to the word "büschel-formig" (= fasciculate) applied to the arms of the Pentacrinite; interpreting this by the various figures given (*e.g.*, of Hiemer's Caput Medusae, 1724; plate i. in vol. iii. of Davila, 1767; the Gmelin Pentacrinite in Knorr, 1755, plates xi. b and c), I suggest that it refers to the presence of numerous armlets borne by the main arm-branches, the structure seen in our type B. No figure of a fossil crinoid of our type C is known to me before 1800.

In 1761 was published the first account of a living stalked crinoid, the Palmier marin, so admirably described and figured by Guettard (1761), and also made known by the curious work of Don Antonio Parra (1787). This was seen to have a stem formed of *Asteriae*, and for this reason no doubt it received from Linnaeus (1767) the trivial name *Asteria*,* when he placed it erroneously in the genus *Isis*. The discovery of this form gave the long-desired explanation of the true nature of fossil Encrinites and Pentacrinites, and since the species resembled the Pentacrinites more closely than the Encrinites, it was by many regarded as "the true original" of the former fossils. This phrase demands explanation. The old naturalists regarded fossils as the representations or images of beings now living on the Earth, and the living *Echinus*, for instance, was the original of the fossil Echinite, just as a picture might be the original of an engraving. Some then, among whom was the learned J. E. Walch, considered the *Pentacrini* with cirri to be imprints of an animal like the Palmier marin. The zoologists do not seem altogether to have accepted this view: those who, as Ellis (1762) and Lamarck (1801 and 1816) compared it with the fossils, referred it to the genus *Encrinus;* while Blumenbach in the various editions of his "Handbuch der Naturgeschichte" (1779 onwards) insisted that the Pentacrinite, though it indeed resembled the Palmier

* P. H. Carpenter (1884) writes *Pentacrinus asterius*, saying (p. 303) "the expression *Pentacrinus asteria*, used by Lütken, Thomson, and myself being a false concord; for it is evident that the etymology of Linnaeus's name *Isis asteria* is the adjective ἀστέριος, starry, and not the noun *Asteria*, cat's eye. I am indebted for this tardy correction to the critical acumen of my friend, Professor F. Jeffrey Bell." I believe that *Asteria* is a substantive in apposition, and that no question of concord of genders arises.

marin, was not congeneric therewith. Blumenbach was well aware that, as he said in 1790, "Hiemer's Medusa-palm (*Helmintholithus portentosus* Linn.) has long had the claim to the name Pentacrinit."

People in those days did not attach such importance to names, priority, and preliminary notices; but even so it is rather surprising that the name Pentacrinite was not used in a regular Latin form and in accordance with the binominal system until 1804. In that year was published Heft 7 of Blumenbach's "Abbildungen naturhistorischer Gegenstände," a work that rigidly employs the Linnaean system of nomenclature. No. 70 of this is the description and figure of a "Medusenpalme" from Dorsetshire, under the name *Pentacrinites fossilis*; as such it is distinguished from *Encrinites fossilis*, first named in the preceding Heft (1802), as well as from the species common at Boll in Würtemberg. This then, a well-known representative of our form B, is the type-species of the genus *Pentacrinus*.

As to the difference of termination, it is perfectly clear that the early writers made no real distinction between *Encrinus* and *Encrinites*, *Pentacrinus* and *Pentacrinites*. The termination *ites* was generally applied to the organism when in a fossil state, but was not held to indicate generic distinction. It is often confused with *lites*, and said to be a corruption of λίθος, a stone; but this is an etymology invented afterwards, like Schroeter's derivation of Encrinos. It is merely the Greek suffix—ίτης, meaning "of the nature of." It has never been maintained that the chance addition of this suffix constituted a difference of name, and all modern writers on Crinoidea have merely dropped it as out of harmony with modern views as to the nature of fossils.

Now, if most early writers drew a correct distinction between *Pentacrinus* and the Palmier marin, how, it may be asked, has the name become restricted to the genus (C) of which the Palmier marin is the chief living representative? There may have been one or two writers who, as Oken (1815), referred *Isis asteria* to *Pentacrinus*; but it was not till J. S. Miller published his "Natural History of the Crinoidea" (1821), that this view became at all general. In his "*Pentacrinites* vel *Pentacrinus*," Miller included *P. caput-medusae* (= *Isis asteria*, Linn., *Encrinus caput-medusae*, Lamarck), *P. briareus* (= *P. fossilis*, Blumenb.), *P. subangularis* (? = *Caput-medusae*, Hiemer, *Helmintholithus portentosus*, Linn.), *P. basaltiformis*, and *P. tuberculatus*. The last two and the first one belong to type C, the others to type B. It was the authority of J. S. Miller, whose fame is indeed well-deserved, that led subsequent writers to follow his example; and the name *Pentacrinus caput-medusae*, though incorrect in both its parts, was further fixed in the minds of zoologists by the classical memoir of Johannes Müller, "Ueber den Bau des Pentacrinus caput-medusae" (1843).

The next step in a wrong direction was that taken by T. & T. Austin (1847), who, while rightly distinguishing types B and C, retained the name *Pentacrinus* for the latter, and gave to the former the new name *Extracrinus*. This name, however, was not accepted by Quenstedt (1874) the chief authority on Liassic crinoids; and in fact the distinction between types B and C was not admitted until the important works of P. H. Carpenter (1884) and P. de Loriol (1886-9) placed it on a firm basis. This was the opportunity for a correct application of the rules of nomenclature. Unfortunately Carpenter chose to exalt the authority of J. S. Miller, and to regard him as the Linnaeus of the Crinoidea. Hence he took as the genotype of *Pentacrinus*, the species first mentioned by Miller, which happened to belong to type C, and for B he adopted the Austins' name, *Extracrinus*. It is worth noting that Miller himself, in the circular announcing the publication of his book, stated that it would include "the genera *Encrinus* and *Pentacrinus* of former Authors." Miller never claimed to be the founder of *Pentacrinus*; his diagnosis of it was in terms applicable only to type B, "the *Pentacrinus* of former authors," and all that was original was the transference of the *Encrinus caput-medusae* of Lamarck to this genus despite the correct general opinion that it was distinct. Moreover, if he intended to date crinoid nomenclature from Miller, Carpenter should have accepted the trivial name *Caput-medusae*, instead of going back to *Isis asteria*, Linn.; also he should, for another genus, have retained the name *Comatula*, instead of going back to De Frémenville's *Antedon*. As a side-issue it may be mentioned that *Caput-medusae* was really the name applied by pre-Linnaean naturalists to the branched ophiurids, the *Astrophyton* of Linck and his successors, and the *Gorgonocephalus* of Leach. The fossil pentacrinid crowns of Boll were supposed by some to belong to this genus (*e.g.*, Hiemer, 1724), but Lamarck's assignment of the name to the Palmier marin merely showed an ignorance of its previous history.

It is now, I trust, clear that the name *Pentacrinus* belongs to type B. There remains for consideration the name to be applied to C. The earliest name found is *Polycerus*, Fischer de Waldheim (1811). A statement twice repeated in this author's rare pamphlet, that "le palmier marin est le vrai original de ce genre," might be taken as a convincing argument in favour of the adoption of this name. But the word 'original,' as already explained, was in no sense equivalent to our modern expression 'type-species' or 'genotype.' *Polycerus* was the "genre que quelques Naturalistes ont appellé avec WALCH *pentacrinite*," and was proposed, in allusion to the numerous arm-branches, simply because Waldheim objected to the name *Pentacrinus* as inappropriate or ambiguous. It is a pure synonym of *Pentacrinus*, and the above-quoted sentence merely

means that in Waldheim's opinion the Palmier marin was the living representative of the genus. He emphasised this, because confusion had arisen, thanks to Ellis, Mylius (1753), and others, between true crinoids and a polyp dredged off the coast of Greenland—the *Isis encrinus* of Linnaeus (1758), *Pennatula encrinus*, Pallas (1766), or *Umbellularia groenlandica*, Lamarck (1801). Waldheim, in short, was far from proposing any distinction between the Lias fossil and the living Palmier marin; on the contrary he was one of the first to refer the latter to the old-established genus.

For an appreciation of the distinction between our types B and C, couched in scientific language and expressed by a correct nomenclature, we must turn to H. von Meyer (1837). This eminent palaeontologist described a new crinoid from the Calcaire corallien of Besançon (Doubs), and, while recognising its affinity to *Pentacrinus* pointed out the following differences: the cirri are round, not compressed rhomboid or oval; there is no downward prolongation of the radials over the stem; the arms fork regularly and equally instead of bearing armlets (figs. V., VI.). The last feature suggested the name *Isocrinus*, the name of the species being *I. pendulus*. It is clear from his description and figures that Von Meyer was dealing with a species of our type C; this was recognised by P. H. Carpenter (1880,-84). Unfortunately Von Meyer was unable to see the sutures between basals and radials in his specimen, and described it as having a base formed of a single pentagonal plate, with no elements corresponding to what we now term radials. This led either to the non-recognition of *Isocrinus*, or to its recognition in a wrong sense, as by D'Orbigny (1849), Desor (1845), and Pictet (1857). De Loriol (1889), however, has re-examined the type-specimen of *I. pendulus*, now in the Natural History Museum of Vienna, and has shown that it possesses five small basals, and five radials. When Von Meyer described the specimen it retained a portion of the stem and cirri, and this no doubt obscured the sutures of the minute cup. Since then these structures, along with portions of the radials and basals, have been destroyed, and the true composition of the cup exposed. Any doubt that may have existed as to the systematic position of *Isocrinus pendulus* is now dispelled. It is congeneric with all species of our type C, and to them the name *Isocrinus* must be extended.

In consequence of the confusion caused by the lumping action of Miller, by the erroneous repartition due to the Austins, by Von Meyer's mistake, and by the slow acquisition of knowledge concerning the structure of the cup in Pentacrininae, various other names were proposed subsequently. *Cainocrinus*, Forbes (1852), of which *Picteticrinus*, De Loriol (1875) is admitted by its own author (1897) to be a synonym, was founded on the presence of a closed basal circlet

a character which, as Carpenter has shown, is of no classificatory value; the species belong to our type C and the names are therefore synonyms of *Isocrinus*. *Chladocrinus*, L. Agassiz (1835), was separated from *Pentacrinus* of Miller in these words: "On pourrait désigner sous le nom de Chladocrinus les espèces dont les rayons accessoires forment des verticilles plus ou moins distans." De Loriol and Carpenter suppose the word "rayons" to refer to the cirri of the stem; but this makes the sentence a mere repetition of "tige portant de distance en distance des rayons simples, verticillés," which is given as a character of *Pentacrinus*. I suggest that "rayons" refers to "rayons du disque," *i.e.*, arms, which may, as Agassiz says, "se ramifier en de nombreux appendices pinnés à leurs bords"; these appendices or accessory branches are, surely, the armlets of our type B. On this view the sentence has a meaning that does not insult the intelligence of Agassiz, while the name *Chladocrinus* is appropriate and antedates *Extracrinus*, Austin. At the same time it is a synonym of *Pentacrinus* (*sens. str.*). *Heterocrinus*, O. Fraas, has been quoted more than once as a synonym of type B. Fraas (1858) did distinguish between two types of branching, "Isocrine" and "heterocrine"; but he carefully explained that he did not intend to propose any new generic name, and he doubtless knew well enough that the name *Heterocrinus* was preoccupied by J. Hall.

The conception of *Pentacrinus* here maintained has been urged by no less an authority than Sir C. Wyville Thomson (1864). Referring to *P. caput-medusae*, he says: "Another and a widely different species, *Pentacrinus briareus*, from the Lias of the South of England, seems, however, to have a just claim to be recognised as the type of the genus *Pentacrinus*." For *P. caput-medusae* and one or two fossils which closely resemble it, Sir Wyville therefore proposed the name *Cenocrinus*. In no case could this name be accepted, since it has the same derivation as *Cainocrinus*, Forbes; and whichever be the correct spelling, in that way should both be written. This question, however, does not arise, since both were anticipated by *Isocrinus*. In the same paper Sir Wyville proposed the separation of *Pentacrinus decorus* and allied species as a subgenus, *Neocrinus*; but the need for this step has been denied by Carpenter.

The conclusions to which we are brought concerning the names applicable to types B and C are as follows:—

B.—PENTACRINUS, Blumenbach, 1804; Type, *P. fossilis* (= *P. britannicus*, Schlotheim; *P. briareus*, Miller). Synn. *Polycerus, pars*, Waldheim, 1811; *Chladocrinus*, Agassiz, 1835; *Extracrinus*, Austin, 1847.

C.—ISOCRINUS, Von Meyer, 1837; Type, *I. pendulus* (= *Pentacrinus amblyscalaris*, Thurmann?). Synn. *Isis, pars*,

Linnaeus, 1766; *Encrinus, pars,* Lamarck, 1801 and 1816, Blumenbach, 1779-1807; *Pentacrinus, pars,* J. S. Miller, 1821; *Pentacrinus,* T. and T. Austin, 1847, P. H. Carpenter, 1884; *Cainocrinus,* Forbes, 1852; *Pictetiocrinus,* de Loriol, 1875; *Cenocrinus* and *Neocrinus,* W. Thomson, 1864.

These conclusions will not be agreeable to those who follow P. H. Carpenter in driving a coach-and-six through the rules of nomenclature, a pastime in which my regretted friend too often indulged. But they will please those palaeontologists who, with the eminent Von Zittel, still call the fossils of Würtemberg and Dorset by the name *Pentacrinus,* a name that has been theirs for three centuries and a half, in the possession of which they were legally confirmed by the illustrious Blumenbach, to whose action Eichwald (1829) is a trustworthy contemporary witness.

With the necessity for some such step I have long been acquainted, but the accumulation of the requisite literature has been a slow process. The general neglect of ancient writers, due partly to the apotheosis of Linnaeus, partly, as Sir Archibald Geikie puts it, to the engrossing interest of the present activities of our science, is a neglect that leads to much misconception, and to much that is not merely unjust but illegal. Because Linnaeus lived, are we to ignore Rosinus, Walch, and Schroeter? Because J. S. Miller wrote a learned monograph, are Blumenbach and Von Schlotheim to suffer total eclipse? Because Johannes Müller turned the light of his genius on the Palmier marin, shall we forget its first admirable description by an equally great genius, Jean Étienne Guettard?

F. A. Bather.

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IV

The Antiquity of the Deep-Sea Fish-Fauna

IT has long been generally admitted that among marine organisms the keenest struggle for existence and the most favourable conditions for the processes of evolution, are to be met with along the shore-line. Those out-of-date forms of life which can no longer compete with the vigorous shore-dwelling races, are compelled to retreat to the freshwaters on the one hand, or to the deep sea on the other. Among fishes the antique mud-fishes (Dipnoi) have thus survived only in the freshwaters of parts of Africa, Australia, and South America; the fringe-finned ganoids, so abundant in the seas and estuaries of Palaeozoic times, are now confined to African rivers; while the higher bony ganoids of Mesozoic age, related to *Amia* and *Lepidosteus*, seem to have become exterminated in the seas of the early Eocene, thence retreating to the freshwaters both of Europe and North America, and at present surviving only on the latter continent. Some of the primitive forms of the higher fishes—the so-called Teleostei—which first appear (so far as known) in the seas of the Cretaceous period, have also survived to the present day solely or chiefly in freshwaters; but many of these races have migrated instead to the dark and gloomy abyss of the deep ocean.

It is interesting to note that, so far as the present state of knowledge permits the expression of an opinion, there is no evidence in the geological record of the last-mentioned refuge before the latter part of the Cretaceous period. There may have been seas of great depth, and there may have been organisms living at the bottom of them; but, if so, none of these pre-Cretaceous sea-beds have hitherto come under the observation of geologists in any part of the world. The chalk and associated strata, however, must have been deposited at a depth sufficiently great to accommodate a fauna of an essentially deep-sea type. It is thus to these formations that we turn for possible forerunners of the inhabitants of the depths of the sea at the present day.

It is, of course, difficult to decide from the fossils of any deep-water deposit, which of the organisms represented have fallen from the surface and which have actually lived at or near the bottom. Moreover, it is quite likely that some of the animals, which inhabit great depths at present, lived in shallower waters so long ago as

Cretaceous times, and have descended more recently. Among fishes, however, it is usually possible to distinguish the deep-water forms by their comparatively delicate skeleton, or by their attenuated fin-rays, or by indications of a great development of the slime-canal system which is related to the production of luminosity or even to the formation of special luminous organs. Judged by these criteria, the majority of the deep-sea fishes of the Cretaceous period are more or less closely related to the Scopeloids and Berycoids, which still form so conspicuous an element in the abyssal fauna. They are best known thus far from the Chalk of England, and from equivalent deposits in Westphalia (4), Bohemia, Dalmatia, and the Lebanon (1).

Some of the Cretaceous Scopeloids with a delicate skeleton—such as the so-called *Sardinius* and *Sardinioides* from Westphalia (4)—can scarcely be distinguished from existing deep-sea members of the family Scopelidae. Others, however, clearly belong to extinct groups, and among the latter may be particularly mentioned the long and slender fishes of the family Dercetidae or Hoplopleuridae. The little arrow-head-shaped, spiny scutes of *Dercetis* are common in the English Chalk, and nearly complete skeletons of the fishes to which they belong are found both in Westphalia and the Lebanon (1). The head and trunk are almost eel-shaped, but the skin is armoured with longitudinal series of the small spiny scutes. Both pairs of fins are present; and the median fins are divided into three, namely, one comparatively extensive dorsal, a slightly forked caudal, and a small anal. The vertebral bodies are not solid, but merely constricted cylinders, and the notochord must have been originally continuous through them. The soft parts, of course, are never preserved; but one specimen of *Dercetis* from the Lebanon, now in the British Museum (No. 49540), seems to prove that some of these fishes at least were provided with a distensible stomach—a feature so common among existing deep-sea fishes, but not hitherto demonstrated among extinct forms. The fish in question is preserved on a slab of lime-stone, and is shown of two-thirds the natural size in the accompanying photograph (Plate X). In the region which must have been occupied by the stomach, it exhibits a comparatively large and deep-bodied fish, with the head pointing backwards towards the tail of the *Dercetis*; and the dermal scutes of the latter are displaced in such a manner that there can be no doubt as to the deep-bodied fish having been swallowed head foremost. Judging from other specimens, the normal depth of this *Dercetis* at the back of the head would be only 10 millimetres, and below the dorsal fin only 13 millimetres; so that the swallowing of a fish 22 millimetres in depth necessitates considerable capability of distention.

Besides being related to the Scopeloids, the Dercetidae of the

SLAB OF CRETACEOUS LIMESTONE FROM SAHEL ALMA, MOUNT LEBANON, SHOWING A FOSSIL FISH (*DERCETIS*) WITH ANOTHER FISH IN ITS DISTENDED STOMACH.
(Two-thirds natural size.)

Cretaceous are also probably somewhat allied to the strange eel-shaped fishes of the family Halosauridae, which are a characteristic feature of the surviving deep-sea fish-fauna. These fishes have a very long tapering tail, with no separate caudal fin, but their dorsal fin is short in the middle of the back, and both the pectoral and pelvic paired fins are present. The body is covered with scales, and those of the lateral line, displaced close to the ventral border, are enlarged to bear a series of luminous organs. It is interesting to note that these unique fishes have also been identified quite lately (6) in the Cretaceous of Westphalia; the so-called *Echidnocephalus* of the latter formation being essentially identical with the surviving *Halosaurus*, and displaying even the enlarged series of scales observed in the latter.

As to the fishes most closely related to *Elops* and the herrings, there are not many in Cretaceous deposits which can be claimed as truly deep-sea forms. Of these, indeed, there is only one of much importance—*Istieus* from Westphalia (4) and the Lebanon (1); but this genus is of the greatest interest, because there are no means at present of distinguishing it from the surviving *Bathythrissa* of the deep sea near Japan. The latter fish is known only by two specimens, one in the British Museum, the other in the Berlin Museum, which can hardly be dissected; and the fossil form, which is abundantly represented in Westphalia, is thus destined to afford the first information concerning the skeleton. It is a fish much like *Elops* in general character, but the dorsal fin is considerably extended and fringes the greater part of the back.

With regard to the Berycoids of the Cretaceous, there is nothing worthy of remark, except that very few of the so-called *Beryx* truly belong to that existing genus. As at the present day, some genera have the aspect of deep-sea forms, while others are as clearly adapted for a shallow-water or surface habitat.

The most striking difference between the deep-sea fish-fauna of the Cretaceous period and that of the existing ocean consists, indeed, in the preponderance of physostomous species in the former and in the complete absence of eels and the Anacanthini—the cod-fishes and their allies. At the present day, according to Dr Günther, the species of Scopeloids form about one quarter of the total fauna, while the Anacanthini constitute another quarter, and the strange forms of eels are an important element. In the Cretaceous fauna, it is scarcely too much to say that (so far as known) the Scopeloids comprise half the species, the other Physostomous fishes at least another quarter, and the Berycoids and their allies the remainder. The only traces of eels hitherto discovered in the Cretaceous are a few specimens of quite normal shallow-water types in the Lebanon (1); and no fishes of this remote date from any part of the

world have hitherto been plausibly interpreted as the forerunners of the Anacanthini.

The next deep-sea fish-fauna after that of the Cretaceous occurs in the black hardened shales or slates of the Canton Glarus, Switzerland, which are commonly assigned to the latter part of the Eocene period (5). Here the higher spiny-finned fishes are already represented by such specialised forms as the ribbon-shaped *Lepidopus*; and there are also true cod-fishes of at least one genus named *Nemopteryx*. There are still no deep-sea eels, so far as known; nor are there any traces of the degenerate angler-fishes related to *Lophius*, such as characterise some of the greatest depths at the present day.

The general conclusion to be derived from palaeontology, therefore, is that the tenanting of the deep sea with fishes has been a gradual process, beginning at latest in early Cretaceous times and gradually proceeding until the present day. As new types have arisen successively in the shallower waters, a few have been driven from the regions of greatest competition to the refuge of the inhospitable depths. At the time when physostomous fishes were the dominant type, the refugees were chiefly Scopeloids, allied families, and primitive relatives of the herrings; in the early Tertiary period, when Anacanthini first appeared in this part of the world, a few cod-fishes were added to the abyssal tribes; while at some later but undetermined period the strangest of all deep-sea fishes—the highly modified eels—must have completed the remarkable fauna as we now know it.

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V

A New Scheme of Geological Arrangement and Nomenclature

Part IV

IN my previous paper I ventured to criticise the current nomenclature and arrangement of the later Tertiary beds as inconsequent and misleading. I will now proceed to a more difficult and hazardous duty—namely, that of proposing a scheme of my own.

It will be understood that at present we are dealing with the latest beds only, inclusive of those now being deposited, that our geographical horizon is limited to the Archipelago of the British Isles and the surrounding seas, and that, consistently with the views I have previously maintained, I propose to treat the land and its fresh waters in one category and the seas in another.

In accordance with the views of some other students, and notably those recently propounded by Dr Hicks, I have always maintained that if we start with to-day and march backwards in surveying the land surfaces of those islands, we shall not arrive at a really substantial halting-place in our geological journey until we reach the base of the so-called Forest Bed. From the base of the Forest Bed until the very latest deposits of silt and mud, of blown sand, of humus or of bog, there has been one continuous stream of unbroken animal and vegetable life in these islands. During this long period some animals and plants have disappeared and become extinct, others have been introduced, and the general facies of the animal and vegetable life has changed considerably, but this has been for the most part a gradual and gentle process. It has not involved a general destruction of all life here, as the older theologians thought, nor the equally fantastic notion of the migration of its animals and plants *en masse* goodness knows how and goodness knows whither, and their re-migration back again in the same condition after a long geological interval, as the wilder Glacial men have urged. The evidence seems to me to be complete and very decisive on this point, and I am very glad it was so strongly emphasised by the President of the Geological Society in his recent address.

Secondly, when we travel beyond the Forest Bed it seems to me as plain as can be that we have a real gap and a real hiatus that points in these latitudes, whatever may have been the case elsewhere, to some very important change in the fauna and the flora of the land. For Britain, therefore, whatever may be the case elsewhere, we must claim that a very natural and logical geological epoch or period is constituted by the series of beds, commencing with the base of the Forest Bed and coming down to the handiwork of tide and river, rain and frost at this moment, and that this forms a very natural substantive and independent division, or series of our beds, marking a very distinct period, and that it marks, in fact, the very last one of all in the geological series.

What name should this period bear? As we have seen, the various names now current have the double infirmity that their meaning and connotation have become utterly confused and worthless, and that they have been applied indifferently to submarine and subaerial beds, which we wish to treat apart. If we are to select a name, it ought to be a name connected with the land and its products, and it seems to me that those names are the best which are taken from some type of animal or plant which marks the particular series both in time and in place. I am not sure that the term Anthropozoic or 'Human period' has not much to recommend it.

I, of course, object, as I have said before, altogether to making the mere advent of man a special reason for creating a new geological horizon, as Lyell and others have done, but it happens that this particular series of beds which (in Britain at all events) has a complete *solidarité* of its own, is also marked by the presence of man and his works all through, and that man is in effect a very good type animal by which to ear-mark the series. If any one can suggest a better name, let us have it by all means. What it is important to secure is the thing itself—viz., a natural and logical nexus of Beds. Names are after all only indices of knowledge, and the best name is that which best discriminates and defines the fact, and which connotes the idea we want to fix with the greatest precision.

Having suggested a generic name for them, let us next try and consider how we are to divide the long series of deposits in question into sections or groups, and let us try to secure some criterion which is not indefinite, but one which marks real changes and real frontier lines.

Apart from the shifting and unstable criteria which have led to the confusion in the commonly accepted nomenclature and arrangement of the later Tertiary beds, there is an underlying difficulty of more critical importance—namely, the actual divergence of opinion in explaining the facts. This divergence is very re-

markable in regard to some facts which are really very elementary, and whose correct interpretation one would suppose was a matter of no great difficulty. But here, as elsewhere in English geology, we are met with an awkward difficulty. An individual geologist who makes a mistake of diagnosis can be easily pushed aside. If he does not choose to perform the happy dispatch, and to correct his own error himself, he and his craze can be jumped upon, and he speedily passes into oblivion. It is not so if the mistake is made by an official geologist. In that case it becomes the foundation of a school creed. Not only have Tom, Dick, and Harry, who come after the original sinner—like the French Staff in the Dreyfus trial—to write memoirs on corresponding phenomena elsewhere, to rub down their eccentricities to one level of official orthodoxy, but they have also to remember that the office and its staff has been already committed to an opinion which must be upheld. Every member of the staff is an Athanasius in *esse* or *posse*. Athanasius having spoken, the Council of Nicæa must uphold his view! and the subsequent memoirs issued under official sanction have to be coloured by this view until some irresistible earthquake arises, like the birth of Dr Hicks or Professor Lapworth, for instance, when the whole set of memoirs on the particular issue go tumbling down together. This sometimes takes a long time. The individual student feels that if his work is to live at all, he must not only work in the field but in the library also. He must not only know what he has done himself, but what other people have done, and he would consider it a piece of impertinence, when writing upon a subject in which others have taken a different view, to pass by that view without notice or criticism, and to write scores of pages of controversial matter with real or affected ignorance of what has been done elsewhere. He cannot take up the position that the only good geological wine is that contained in official bottles and that the official seal is as conclusive as veuve Clicquot's corks.

This is a very fine position to take up, so long as the public appealed to is that which always requires an official seal for its information, and is content with no other test, but a good many people are not made that way, and among them are, perhaps, some readers of this Review, who see perfectly well that the particular policy in question must necessarily have its Nemesis.

To come down to concrete issues, it seems to some of us that the really important horizon to fix, if we are to divide the Anthropozoic period as above defined, is the real position of the Drift beds; the beds once known as Diluvium, and now very often spoken of as Glacial by a school which was some time ago a great deal more cocksure than it is now. Lyell's term drift is a neutral one. Do

the drift beds lie over or under the beds in which the Mammoth occurs, and in which so-called palaeolithic man has left his traces? Upon this issue a great deal has been written which does not seem to have penetrated official ears, nor, perhaps, the conservative atmosphere of Burlington House. Now that the subject has been made the special matter of an annual address from the president of the Geological Society, perhaps it will receive a little more attention.

To some of us who have diligently collected and published the evidence for many years past, and I myself have been allowed to do so at great length in the *Geological Magazine*, thanks to the evergreen benevolence of my old friend Dr Woodward, there remains little or no doubt that in every case where the evidence is clear, in the Old World, and probably in the New, the Drift beds plainly and distinctly overlie the so-called Mammoth beds. This was the view of Falconer and John Phillips. It is the view of Mr James Geikie and Dr Hicks, with neither of whom do I agree on all matters, but who on this one have done yeoman service in illustrating the question. We have not been content with mere rhetoric; we have shown in detail and by an immense concurrence of facts that this is the case, and that when the facts appear to lead to a different conclusion, the bones or other *débris* enclosed in the drift are as much boulders in it as the marine shells have been shown to be and as the travelled stones are.

No attempt has been made to seriously answer the case thus made out, but with the persistent monotony of that erratic bird the cuckoo, we have had on the other side one echoed and re-echoed assertion in the form of an *obiter dictum*. If there was ever an instance in which the burden of proof was upon the other side, it is this one. It is not surprising, therefore, if there has been some resentment about it and some impatience, and if in my hearing some phrases have been used about it, which had better be left to the imagination.

The views which have prevailed about the true position of the Mammoth beds in regard to the Drift are virtually three. One view has been that the Mammoth lived entirely after the distribution of the Drift. This view is, I believe, extinct. The evidence is too overwhelming that in numberless places the remains have been found in undisturbed beds below the drift for this view to be any longer tenable. A considerable number of people still maintain, however, that the Mammoth both preceded and outlived the Drift phenomena, having thus lived in two epochs, whence the name Dicyclotherium given to the beast by Geoffroy St Hilaire. I have tried with all the patience and care I could to examine the various finds of Mammoth remains in the northern hemisphere where there

is any evidence of succession, and I claim to have shown that in no instance, so far as we know, does the Drift actually underlie any land surface containing the remains of the Mammoth and of its contemporaries (*Geol. Mag.* for 1892-1893 *passim*).

No doubt Mammoth's teeth and bones, and the remains of its companions, have been found in boulder clay and in pockets intercalated in that clay; so have ammonites and belemnites and bones of Liassic saurians of coal plants, of Triassic shells of Carboniferous corals, etc., etc. But in all these cases the remains in question are adventitious, and have been collected by the force, whatever it was, that distributed the clay, and they occur in the Drift beds as boulders, just as certain broken trunks of trees do. To call the remains of the Mammoth when so found interglacial, and to refuse the same style to the ammonites and belemnites, is inconsequent. Nay, more, it would be almost incredible if some of those massive teeth and bones had not occurred as boulders in the drift, since they must have been lying about on the old land surface over which the drift was deposited.

The fact is that there would not have been any question raised about the matter in these later days with the present knowledge we possess if it had not been that a certain number (a rapidly diminishing number, if I am to judge by the conversations and correspondence I have had lately) of geologists are committed to the purely *a priori* theory of interglacial periods, which theory again was the product of the now entirely discredited astronomical theory of an ice age. It is the desperate straits to which the champions of interglacial periods are driven that makes them cling to these boulder-bones and teeth as evidences of so-called interglacial beds. And this clinging is emphasised by the fact that more inconsequent, unscientific, and childish nonsense seems to me to have been written on so-called interglacial beds in the memoirs of the Geological Survey dealing with the surface deposits than upon any other subject. The term is used over and over again in these memoirs without any attempt to justify it, as if there was any general concurrence of opinion to support it, whereas if the geologists of the world were polled, the number of believers in what is a mere hypothesis, eventually based upon Croll's now exploded astronomical theory of an ice age, would be found to be a mere handful. Surely we have a right to complain that in Government publications, paid for by the taxpayer, and carrying the authority of official documents, an hypothesis so generally discredited and so generally discarded should be so calmly assumed as if it had the authority of an axiom.

No one objects to the theory of interglacial periods, or of the possibility of squaring the circle, or of extracting sunbeams from cucumbers being defended in private and individual memoirs like

"Gulliver's Travels." What we object to is these fallacies being published as the State geological religion.

It is really too amusing how far the matter is carried. At a recent meeting of the Geological Society a paper was read upon the never-ending problem of the relation of the Drift to the Mammoth beds in the caves of North Wales. It was shown by the author of that paper, Mr Pollen, very conclusively that the Drift overlies the beds contemporary with the Mammoth in the particular case in question. Therefore my friend, Mr C. Reid, who has done an immense amount of careful and very valuable observation, to which I personally am under great obligations, but who is an arch-sinner in regard to the championship of transcendental theories, at once suggested that the beds in question were interglacial. "Interglacial" to most people means "placed between two glacial beds," but this is not its meaning apparently when used by Mr Reid. To him a bed becomes interglacial if it is merely covered by Drift. It was a curious epilogue to the discussion in question that Mr Strahan, who has surveyed and mapped the surface beds of North Wales with singular care and skill, should have got up and declared very positively that there is not the slightest trace of interglacial beds in North Wales at all.

I must be allowed to quote another amusing example. So-called palaeolithic man, as is well known, was the contemporary of the Mammoth. But for some reason a certain number of writers committed themselves long ago to the idea that palaeolithic man did not live before the distribution of the Drift, but after it. The evidence drawn from the Mammoth beds, etc., however, was getting very awkward, and pointed very definitely the other way. The British Association was accordingly invited to make a grant to test by an excavation the famous typical site at Hoxne, where the evidence had been read in different ways by different explorers. Some of us gladly welcomed the appointment of a committee, but were taken very much aback when we found that the only men put upon it were those already committed (may I say violently committed) to the view that the implement-bearing deposit at Hoxne was newer than the Drift. This was not very promising, nor was it quite judicial. It was, in fact, indecent. When the report came out our suspicions were more than justified, for a more extraordinary discussion and summing up of a serious polemical subject was perhaps never seen.

This is not the occasion to discuss the Report in detail. That may be done on another occasion. At present I would merely refer to one point (perhaps the strongest argument used) as a sample of the kind of arguments used. It is well known that in East Anglia the crag beds occur in more than one form, and in most places in

the form of barren red or yellow sands. When these sands occur in a somewhat disturbed or dishevelled form, they have been called "Middle Sands," and have been associated with the Drift. Why, I know not, for they are essentially crag sands, and Mr Horace Woodward has shown how difficult or impossible it is to separate them from true crag. When the principal pit was dug at Hoxne the other day, the supposed equivalent of the beds containing palaeolithic implements was found to be underlaid, not by boulder clay, either brown or chalky, or by any deposit containing foreign erratics, but by a bed of barren sand (unmistakably, as it seems to me, for I know the neighbourhood) belonging to the crag series. It is curious, notwithstanding all this, that the bed of sand in question has been labelled Glacial Sand on the diagram attached to the report! And in this way the palaeolithic bed is made out to be inter-glacial or post-glacial.

I shall not pursue this matter further, but content myself by reaffirming that the very numerous facts quoted by Dr Hicks and myself and others, prove that the Mammoth bed or beds, with palaeolithic implements in them, in all cases where they can be properly tested, underlie the Drift beds. Those who say they don't had better answer our facts and arguments instead of shouting cuckoo to each other.

The next point upon which I entertain absolutely heretical views is as to the origin of these beds. In my view they have been distributed by water and not by ice. Upon this issue I have printed two considerable volumes, and many papers in the *Geological Magazine*, and every day strengthens me in my opinion. I further hold and think that I have proved that of the contents of these beds the greater part are derivative. This is the case with the rounded boulders, with the rounded and smoothed pebbles in the gravels, and with the sands and the older clays incorporated with them. It is so, as Mr Horace Woodward has shown, and as is now admitted by Mr C. Reid, in regard to the marine shells which were formerly styled glacial, and it is so with the logs of wood, the deer's antlers, and the molar teeth, and great limb-bones of the Mammoth and other animals found with them. All the contents of the beds, so far as we can make out, are adventitious and derivative. This I have maintained at great length elsewhere.

Secondly, I also maintain that these beds, instead of representing a long period with manifold changes, glacial or otherwise, represent a transient and rapid diluvial movement. In the great kames of the Lancashire valleys as on the cliffs at Cromer, the laminae of sand and finely levigated clays are arranged in immense curves reaching sometimes from the base to the summit of the beds. These curves are unbroken and continuous, and are therefore clearly the product

of one impulse and not of a series lasting many ages. The unworn character of the teeth and bones and shells, all derived and found at different horizons in the beds, are also inconsistent with anything but a single and violent impulse. For these and other reasons which I have elaborated at length elsewhere, I must conclude, as I have said, that these beds do not represent any prolonged period, but a transient movement, a transient movement coincident with the breach in continuity between England and the Continent.

These last conclusions are, I know, far from being shared by the geological prophets of these later days. They were the views, however, of my masters—Murchison and Sedgwick, Conybeare, and Whewell, Hopkins and Phillips. Where are better men to be found now? Where, in fact, are such men to be found at all now? They nearly all wrote, be it remembered, after the glacial craze had been published, and all repudiated it. The views in question will, I am convinced, again become the views of another generation which is rapidly abandoning the extravagant teaching of Agassiz and his scholars.

Whether true or false, however, they do not affect the earlier part of this argument, which is supported by a large majority of the geologists of the world—namely, the fact of the priority of the Mammoth beds to the Drift. The Drift, in either case, forms a great "divide," a great frontier, between two main divisions of the Anthropozoic period.

To myself it separates an antediluvian from a post-diluvian period. These terms, however, are ambiguous, since they might be accepted as in some way associated with the ideas of the theological geologists with their Noachian or universal floods, an idea far from any views of mine, and I would therefore apply some other nomenclature to them. I dislike the words palaeolithic and neolithic as geological terms whatever virtue they may possess in archaeology. The former use of stone instead of metal by man was an archaeological accident. Nor does the term neolithic include any but a fraction of the beds which overlie the drifts.

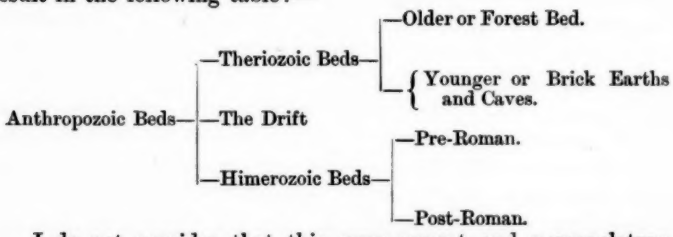
I prefer to differentiate these beds by their animal contents, which are much safer criteria than the different shapes of chipped axes, whose types sometimes overlap. The great cardinal difference of the animal life before the drift and that subsequent to the drift is the presence of domesticated animals in the latter and the presence of wild animals only in the former, and by this test I would separate them, calling the former *Theriozoic* and the latter *Himerozoic*, the one connoting that division of the Human period in which wild animals alone lived with man, and the other the corresponding period in which tame ones did so.

Let us proceed again. The Theriozoic or praedrift beds were, so far as we know, continuous with each other, and if the geological

record was intact in these realms the evidence would show it, but unfortunately the debris of the land fauna and flora at all periods, save one, are broken and fragmentary and scattered. The one exception is that of the fauna contemporary with the Mammoth, which abounds in enormous quantities in all parts of the world. In fact the contrast between this poverty in other beds and the abounding richness in the Mammoth beds is one of my strongest arguments in favour of the catastrophic destruction of the latter fauna, as compared with the slow and gradual decay of its predecessors. This slow and gradual decay, in which bones and shells, instead of being rapidly covered up, as they were in the case of the Mammoth and its companions, were exposed to rain and snow, and faded and weathered away, have left us only shreds of the former life in certain fortunate localities, and there is apparently a considerable gap in our evidence, which compels us to separate the Theriozoic beds into two series,—one of them known as the Forest Bed, occurring in a very limited locality near Cromer in Norfolk, and represented also by some scanty remains in Dorsetshire; the other represented by the contents of the older caverns, the brick-earths of the Thames valley, etc., and generally by the remains of the old land surface immediately underlying the Drift. The former is marked by certain mammals which do not occur in the latter, and which had been probably driven away by increasing cold or otherwise, and had at all events become extinct in this country; and the latter marked by certain animals which had apparently migrated hither in consequence of the increasing cold. Fresh discoveries, however, yearly add to the list of mammals once thought to specially mark the later beds, and which are now found to have existed also in the earlier ones; and in view of possible discoveries in future I should be disposed to simply discriminate the two sets of beds by the words older and younger,—the former especially marked by the presence of *Elephas meridionalis*, *Rhinoceros etruscus*, *Ursus arvernensis*, *Trogontherium cuvieri*, and by a series of deer which in my view have not been yet quite sufficiently studied, but which were apparently limited to this horizon. The latter was perhaps best marked by the introduction of certain northern beasts, such as the Reindeer, etc., and by certain Steppe animals like the Saiga antelope.

Turning to the *Himerozoic* beds, those commencing with so-called neolithic times and coming down to to-day, they have sometimes been classified as prehistoric and historic. I cannot attach any meaning to these terms which is admissible in a scientific classification. We have long ago got past the stage of looking upon history as necessarily based upon written documents. A great deal of all history is in fact archaeology, and it matters little in reconstructing the story of a people whether we piece together our

description of their appearance, surroundings, habits, etc., from their art remains or their written records. I don't know where in fact to draw any rigid line between what is prehistoric and what is historic. Nor do I think the introduction of writing can be accepted as marking a geological horizon. On the other hand, I confess, however paradoxical it may look at first sight, that I know of no better horizon where to divide the beds in question than the invasion of the Romans. This, I know, does not sound like a geological test, but it really has a certain geological meaning. It was the Romans who first began to import into our fauna and flora a great many of our non-indigenous animals and plants, our fruit trees, and also some of our best known forest trees, our garden plants, and perhaps some of our best known strains of domesticated animals, and if we are to choose a dividing line on biological grounds, I know of none better than this one. To sum up, then, this attempt to arrange the subaerial beds of England, which intervene between the base of the Forest Bed and to-day, I would condense the result in the following table:—



I do not consider that this arrangement and nomenclature in any way interfere with the arrangement and nomenclature of the later beds prevailing in the text books, since it is entirely confined to the subaerial beds which have hitherto been mixed up and confused with the submarine beds whose arrangement ought to be dependent on entirely different considerations. To these latter alone the terms Pliocene, older and younger Pliocene, Pleistocene, etc., were originally applied; and if these terms have any virtue to them they ought to be limited, and I propose to consider them and to fill up some gaps in the present paper on another occasion. In conclusion, it is necessary that I should apologise for the too frequent use of the first personal pronoun in the foregoing pages. It is the inevitable consequence of polemics of this kind. I feel sure it does not mean that I am more cocky and self-asserting and impertinent than the friends I love to criticise, and whose criticisms are so welcome to me. If I did not greatly value their work, mistaken as I deem some of it to be, I should not be creeping away from the House of Commons, at the risk of being abused by the Whips, in order to write this paper.

HENRY H. HOWORTH.

SOME NEW BOOKS

PROTOPLASM

THE LIVING SUBSTANCE AS SUCH: AND AS ORGANISM. By Gwendolen Foulke Andrews (Mrs Ethan Allen Andrews). Supplement to the *Journal of Morphology*, Vol. xii., No. 2. Boston: Ginn & Co., 1897.

THIS attractive and well printed volume departs from a recognised tradition in an unusual fashion. It contains an intolerable deal of sack but a very excellent pennyworth of bread. Let us be rid of the sack first. The language of the volume is singularly repellent: it is not British English, nor American English nor Germanised English, nor in fact any of the recognised varieties of English adopted by scientific writers: it is a curiously inflated and lyrical jargon occasionally rising into a pretty but misplaced rhetorical eloquence, but frequently taking revolutionary views of grammatical construction. The treatment resembles the style: it is diffuse and pompous; there is no coherent argument, and a mania for irrelevance and repetition. The good lady insists upon magnifying the application of her observations: from her investigation of the minute structure of protoplasm and of the filose pseudopodia of Gromia she passes gaily to theoretical conclusions concerning habit and heredity and natural selection. To choose a sample of the book, the old method of divination has been followed exactly; a hat-pin was stuck between the leaves and its point indicated the following passage:—"Fosterhood. (146) The word parasitic having by a certain frequent use associations which in some connections are jarring, it should suffer as idea a transformation of verbal form gracious enough to follow that of the fact into the beautiful phenomena of parenthood. Dependent perpetuation areas may be called fosterling areas; the supporting substance foster substance, area, organ, or organism. Of all areas differentiated to live at expense of other parts of an organism, there are none so grossly egotistic, none which so take all and keep all for themselves as the perpetuation or fosterling areas. From their inception, for variable periods, often covering the whole term of their existence, they receive largely from the foster-substance in many of its phases." For the life of him, the present reviewer, who has enjoyed the advantage, likely (he fears) to be unusual, of reading this passage, not only as isolated but as part of the whole volume, cannot see that this means more than that if a protoplastic area is fed from another protoplastic area it receives its food from that other area. The same kind of criticism may be applied to very large portions of the volume: there may, of course, be deeper meanings and a wealth of veiled conclusions, but the bewildering infelicity of the style has obscured them from at least one willing mind.

None the less it is certain that Mrs Andrews had valuable and

novel things to say. She has spent long years on the patient study of living protoplasm: she has compared the kaleidoscopic changes in living cells and nuclei with the arrested, possibly distorted, phases of them seen in fixed and stained preparations. Apparently she has unusual keenness of vision and patience, and has thought no trouble too much did it secure a glimpse into the life of living matter. Had she drawn a great deal more and written a great deal less, knowledge were more surely her debtor. As it is, she has secured many valuable observations.

In his main idea, Bütschli has found corroboration from Mrs Andrews. Protoplasm is a froth or foam or emulsion. It consists of closely packed bubbles, the contents of which are different from their walls. Mrs Andrews, however, believes that the vesicular structure extends far beyond the limits of Bütschli's observations, and that the visible bubbles grade into series so minute as to appear homogeneous even under the highest possible magnification. Moreover, while Bütschli was inclined to attach particular importance to the contents of bubbles, and to regard many of the phenomena of protoplasm as the result of mechanical and chemical relations between the bubble contents and surrounding fluids, Mrs Andrews is more of a vitalist and regards the stuff of the bubble films as the real protoplasm, the active living agent. She describes a large number of cases in which changes of shape, visible under the microscope, cannot be attributed solely or even chiefly, after the manner of Bütschli, to chemical changes between the contents of the bubbles and the surrounding media. Those who have watched Bütschli's artificial foams know that when they are brought into a medium different in chemical constitution from the contents of the bubbles, osmotic changes are at once set up and these result in curious pseudopodia-like protrusions from the surface of the foam, in streaming movements within its mass and even in movements of translation of the whole mass. Many of these changes are exceedingly similar in appearance to the movements of real protoplasm, and there have been many who have gone beyond Bütschli and roundly declared the causes of the movements of life to be similar in kind to these results of diffusion currents. Mrs Andrews, however, describes many cases in which the conspicuous feature of vital movements was actual shifting in position of the continuous substance which forms the walls of the bubbles. She describes that as flowing from place to place, now thickening the walls of the bubbles in one region, now withdrawing itself into invisible tenuity. Moreover, she shows reason to believe that this continuous substance itself may have a foam structure far minuter than anything Bütschli has described. Upon Bütschli's basis, she has increased for us the complexity of the conception of protoplasm, and has shown that a large part of living movement may be the result of chemical differences and emulsion currents within the protoplasm.

Mrs Andrews has made and recorded a specially interesting set of observations on the outer surface of living cells. She declares that in most cases the living matter extends in an attenuated form far beyond what is usually figured and described as its limit. The flickering edge of a cell is usually a net-work of delicate protrusions, ceaselessly changing their position and form. These are in a state of

most active sensibility, and the slightest mechanical or chemical stimulus causes an instantaneous change on them. Moreover, the interior of living protoplasm is in a constant state of assuming local differentiations which are as ephemeral and transitory as the movements of the outer edge. The authoress insists, with reason, that not sufficient attention has been paid to the changes that killing and staining agents must produce upon so sensitive a mass. They must act first as drugs, however rapid their action may be, and the preparations of microscopists may be chiefly pathological conditions.

We hope that Mrs Andrews may be induced to publish figures of a number of the interesting observations that she has made. If structure can be described, it is possible to figure it, and much that is at present obscure in her text might become of great use to other workers. A sentence from the 'conclusion' will explain shortly the general attitude of the authoress to protoplasm. "The facts seem to warrant present belief that the living substance of all organisms is one physiologically continuous, living, plasma, homogeneous throughout in its living powers and properties, but having varied local and temporary habits of self-expression, which are largely and inextricably correlated with physical and chemical conditionings of its form and composition as complex emulsive foam—yet not to be wholly identified with or explained by these."

Allies of the Worms

Traité de Zoologie Concrète, V. Les Vermidiens. By Y. Delage and E. Hérouard. 8vo, pp. xii. 372 and 46 coloured plates. Paris: Schleicher Frères, 1897. Price 25 francs.

We welcome this volume of the "Zoologie Concrète," which, although the second to appear, is really the fifth of the great work undertaken by Messrs Delage and Hérouard. The first volume was published last year, and reviewed in *Natural Science*, p. 200, vol. xi. Under the title of "Les Vermidiens" the authors now deal with a number of interesting groups of Invertebrata, at one time or another considered to be more or less closely related to the Annelids.

First of all the Gephyrea are described, then the Bryozoa. *Phoronis*, *Rhabdopleura*, and *Cephalodiscus* follow, forming a new class, the Axobranchia. The Rotifera and Gastrotricha come next as the class Trochelmia; the isolated genus Echinoderes is placed in a separate class, the Kinorhynchia. Finally, we have the Chaetognatha and Brachiopoda.

We need not again discuss in detail the very original plan adopted in describing these forms—it is essentially the same as in the previous volume. After the anatomy, physiology, and development of the theoretical "type morphologique" of each class has been given, and the types of the subclasses, orders, etc., have been as far as necessary similarly treated, a description accompanied by figures is given of the chief characters of every known genus. Some idea of the completeness of the work may be gathered from the fact that the whole volume contains nearly 400 pages, illustrated by 523 figures in the text, and 46 coloured plates. The Rotifers alone occupy some 40 pages, 10 plates, and 60 figures in the text.

Near the end of the volume are general discussions on the

characters and affinities of the Vermidea, a review of the range of variation in the anatomy and physiology of the group, and some useful synoptic tables of the classification. Finally, there are a bibliography, an index to technical terms, a list of the hosts of the parasite forms, and an index to the genera described in the volume.

Throughout the book the style is fresh and interesting, the descriptions lucid, and, considering the immense amount of detail dealt with, very accurate. It may be pointed out, however, that the "tentacles" of Sipunculus do not form a simple circle round the mouth as described and figured on pages 12 and 16. They are the lobes of a horse-shoe-shaped fold—probably a lophophore in fact—a matter of some importance. The description of Sternaspis is neither as accurate nor as up-to-date as we should like to see it. Considerable vagueness prevails with regard to the nomenclature of the excretory organs and genital funnels. Whatever may be the authors' opinion concerning the homology of these organs (and we hope it will be much more clearly expressed in subsequent volumes), surely the term "organes" and "entonnoirs *segmentaires*" is singularly inappropriate when applied to the nephridia or genital funnels of such unsegmented animals as the Rotifera and Polyzoa. On the whole, the descriptive part of this volume seems to us excellent, and bears witness to the great industry and ingenuity of the authors. Particularly clear and well illustrated are the accounts of such complicated processes as the budding in the Bryozoa and the development of the Ectoprocta.

Let us now turn from the purely descriptive to the more general parts of the volume, those parts which deal with the classification and affinities of the animals under consideration. Here Messrs Delage and Hérouard seem to us to have been much less fortunate. In the class Bryozoa, for instance, the authors still include the Entoprocta, although they are naturally enough unable to construct a real "type morphologique" common to the Entoprocta and the Ectoprocta or Bryozoa proper. It is difficult indeed to see how with any amount of ingenuity they could possibly synthesize these two groups which differ in every essential of their structure. Whilst in the typical Ectoprocta, as is well known, a lophophore bearing hollow tentacles surrounds the mouth, there are no nephridia, and the coelom forms an extensive body-cavity communicating with the exterior by genital funnels; in the Entoprocta we find a ring of solid tentacles encircling the body so as to surround both mouth and anus, a substantial parenchyma in which lie a pair of true flame-cell nephridia, and the coelom is represented by a small pair of genital sacs opening by two funnels to the exterior. True, budding occurs in both groups, and in both the larvae have a more or less vague resemblance to a trochosphere; but the buds are of different nature, and what group of Invertebrate Coelomata is there the larva of which does not bear a certain likeness to a trochosphere? Moreover, recent researches have rendered very probable the view that, whilst the Ectoprocta are fixed on their ventral surface, *Pedicellina* lies, so to speak, on its back. In this case even the nervous ganglia of the two forms would not be homologous. Taking all these things into consideration, we fail to see what excuse can be found for placing the two groups under one name.

The class Gephyrea is another case in point. In spite of all the evidence which has been brought forward to show that *Sternaspis* is a highly modified Polychaete, probably nearly allied to the Chlorhaemidae, it is again placed with the Echiurids. Now the chief, if not only, real differences between the Chaetopods and Echiurids are the presence in the latter of a peculiar preoral proboscis, and of posterior organs with ciliated funnels—these are both absent in Sternaspis. The paired anterior processes described by Sluiter do not contain the cerebral region of the pericœsophageal nerve ring, and appear to be comparable not to the proboscis of *Echiurus*, but to the palps of Polychaetes. The brain of *Sternaspis* is situated in a prostomium, in normal Polychaete fashion. Perhaps more striking still, in such a *fin de siècle* zoology as this, is the retention of the Echiurids and Sipunculids in one class. An attempt to build up a 'type morphologique' possessing the structure of both these forms can only result in the production of a caricature of an animal having the essential characters of neither.

With regard to the new group Vermidea, we were at first inclined to believe that the authors merely introduced the name as a convenient but loose term, somewhat as we use the word Invertebrata. Such, however, is not the case. Messrs Delage and Hérouard claim that the various animals they have brought together by a process of *épuration des vers* are really closely enough related to form a true group. Discarding the Molluscoida and the Podaxonia, and indeed paying little attention to the important work recently done on the subdivision of the coelom in these forms, they make the artificial assemblage of Gephyrea into a central group, having *des affinités réelles* on the one hand with the Annelids, and on the other with the Chordata. From the point of view of phylogeny, such a position seems to us quite untenable. The fallacy of the method may be illustrated by an exaggerated example. Suppose a systematist to be classifying the mammalia; what should we think of his system, if, having first united the Cetacea with the Sirenia and Pinnipedia, he then proceeded to argue that this new group has obvious and real affinities on the one hand with the Carnivora, and on the other possibly with the Ungulata! Yet this, it seems to us, is just the method pursued by the authors in the case of the Gephyrea and Vermidea. The process of *épuration*, or sifting, may be very good for the true Worms, which are not treated of in this volume; but if we pour all the rejected forms into one group, the Vermidea, the classification is left in a worse condition than before, the confusion is worse confounded.

If we have insisted somewhat at length on what seem to us defects in the taxonomy of this volume, it is because we believe no greater mistake can be made than to consider classification as a matter of secondary importance. If rightly understood, classification is not only a valuable summary of the knowledge acquired, but also a guide to farther progress. However clearly and accurately described, facts lose half their value and interest if not presented in their proper relations. When the Bryozoa were shown not to be Hydroids, when the Brachiopods were shown not to be Molluscs, when the Rotifers were shown not to be Infusorian Protozoa, when the Sipunculids were

shown not to be Holothurians, real advances were made, not only in classification, but also in the knowledge of the morphology of those animals. The attempt to establish an obviously polyphyletic group like the Vermidea, containing such forms as *Sternaspis* and *Lingula*, Rotifers and *Phoronis*, *Echiurus* and *Cephalodiscus*, can only, we think, retard the progress of zoology, and confuse the points at issue. To the authors' exclamation that "*les Molluscoides ont vécu*," it can only be answered that we hope the Vermidea, as a group, will never live.

Throughout the volume the numerous figures in the text are both new to text-books, and good; many of them are much improved by the use of colours. Quite a new feature, of course, is the introduction of plates with highly-finished coloured figures. Always instructive, and generally original, these plates are often excellent in conception and execution. We may note especially those dealing with the Bryozoa and Rotifera.

In this work Messrs Delage and Hérouard have successfully kept up the high standard of excellence set in their first volume on the Protozoa, and this notwithstanding the many and great difficulties they must have met. All teachers and advanced students of zoology will be grateful for such a reliable guide to the study of rather obscure groups of animals, generally somewhat neglected in text-books, and will look forward with eagerness to the appearance of the next volume of the *Zoologie Concrète*. E. S. GOODRICH.

AN AMERICAN TEXT-BOOK OF GEOLOGY

AN INTRODUCTION TO GEOLOGY. By William B. Scott. 8vo, pp. xxvii + 573, with 169 figs. and 12 special plates of fossils. New York: The Macmillan Co., 1897. Price 8s.

FROM the first glance there is a refreshing air of newness about this beautifully printed volume, giving a good impression which improves on further acquaintance with the work. It is copiously illustrated, not only with the usual diagrams, but also with exquisite photographs of scenery and special features of geological interest; and it is further embellished with twelve plates of typical American fossils, drawn by Mr Rudolph Weber in his well-known effective style. It is essentially an American book for American students; but the chapters on physical geology can, of course, be appropriately used for teaching purposes in any country, while those devoted to historical geology, though essentially from the American standpoint, are characterised by a breadth of view which is unusual in a geological handbook of a strictly elementary character.

Dr Scott has for many years held the Professorship of Geology and Palaeontology in the University of Princeton, New Jersey, and the arrangement of his book is the outcome of considerable experience as a teacher. After a few introductory remarks, there is a chapter on the rock-forming minerals. Dynamical geology follows, the igneous agencies being first treated, then the surface agencies, and the latter classified under the headings of destructive and reconstructive processes. Structural and physiographical geology form the subject of the next two sections; and historical geology occupies the final 200 pages of the book.

Dr Scott treats each period of geological time in a definite order. He begins by mentioning the origin of the name applied to it, and gives a tabular list of the American strata representing the period. He then proceeds to describe these rocks and the geographical conditions under which they must have been formed; and finally he refers in a delightfully broad manner to their foreign equivalents. The life of the period is then systematically enumerated, with occasional brief descriptions of specially important types; and there is the plate of sketches of common fossils to guide the student in his practical work.

Professed geologists and palaeontologists will naturally turn to Dr Scott's account of the Tertiary formations, to our knowledge of the mammalian fauna of which he has made so many important contributions. Here they will not be disappointed; for, although the chapter is necessarily very brief, it gives in an authoritative manner just such a broad view of the subject as an ordinary student of geology or vertebrate zoology requires. Our only complaint is that the author should still make use of the misleading term 'Quaternary' for the Pleistocene period. He, himself, indeed mentions the fact that the transition between the Pliocene and Pleistocene deposits is perfectly gradual; it would, therefore, have been more logical to refer them to one and the same great Tertiary series. He is to be congratulated, however, on the brevity of his reference to the speculations on the Glacial epoch, which often occupy an undue space in elementary text-books; and his table of strata at the end is also commendably brief and free from bewildering detail.

Dr Scott has indeed produced an elementary text-book of geology of which the University of Princeton may feel proud. It is clearly the work of a teacher, an experienced original investigator, and of one whose knowledge is far beyond the once-common parochial stage.

The Geology of Cambridgeshire

A Handbook to the Geology of Cambridgeshire. [Cambridgeshire Natural Science Manuals.] By F. R. Cowper Reed. 8vo, pp. x+276. Cambridge: The University Press, 1897. Price, 7s. 6d.

Cambridge, fosterer of all sciences, mother of famous geologists, ought to give the world a model Handbook of local geology. What should be the aim of such a Handbook? Not, certainly, to include all that has been written, and so save a student from study; nor yet to be a mere description of the geological map, assigning each area to its stage. A more usual object is to catalogue the formations which occur, indicating the places where they may be examined, and noting the peculiarities which they present. A higher ideal would be to make a geological history of the district, describing its character as land and sea in each successive age: with this might well be combined remarks on the present visible surface, as resulting from its history and its structure. But such a book should not teach general principles, nor debate unsettled controversies. If the writer be great, he may hope to attract students to the science, but if he is wise he will not attempt to teach it to them. The author of this book seems to set before himself several of these aims, and to reach some of them. He, for the most part, follows the same lines as Woodward's

"Geology of England and Wales," but he hardly equals its self-control in adherence to a plan.

The book opens with a physical description of the county; not a very happy one. The Cam Valley breaches higher ground and carries the drainage of a basin into the wide plain of the Fen-land. When the author calls this a division into four parts, he sets before us separation instead of unity. He gives a good account of the relation of its water channels to its earth-structure, but why force upon his readers the most newly-invented jargon of technical terms? We object to obsequious or consequential persons, and to 'obsequent' or 'consequent' rivers.

The papers of Teall, Keeping, Roberts, W. Hill and Jukes-Browne, with the Survey memoirs, have added largely to our knowledge since the time of Dr Bonney's graphic sketch. The author makes good use of them, and his account of the Secondary formations seems in general good and full. He once or twice ventures on a little geological history, in describing the Lower Greensand as accumulated in a narrow strait flanked by Palaeozoic rocks on the east (the latter statement is given without the grounds for it), and again in connection with the Red Chalk. Generally, however, this highest form of description is entirely left alone. "The Portland and Purbeck Beds are wholly wanting and perhaps never existed in this part of England." "All the Tertiary beds are now absent." What was the condition of Cambridgeshire in these periods? land? shore? or still, pellucid sea? Silence is strictly maintained. Silence is also maintained as to the buried Palaeozoic floor. We know of no deep boring in Cambridgeshire; still, in the present interest about possible Coal-measures, some persons may consult this handbook for information, and consult it in vain.

The Pleistocene Deposits are treated at considerable length. Few who take ice in hand can keep cool heads; we have fifteen pages on the "Mode of formation of the Chalky Boulder Clay." The author gives a temperate account of the different theories, but what place have such theories in such a book? Why does he not also discuss Permanence of Ocean Basins in connection with the Chalk, or Formation of Coral Reefs when upon Upware? He gives just such a review of the theories as a cautious Professor would give to thirsters after certainty, but occasionally he strays into positivism. "The chalk floor beneath the clay does not show any striated or polished surface, owing to its softness and the removal of the original surface by subterranean denudation." He probably meant to write 'perhaps owing'; chalk fragments in the clay itself are abundantly striated. We should not have expected that more than one writer could be found who would divide the theories 'into two groups (i.) the glacial, and (ii.) the non-glacial'; this second group consisting of Sir Henry Howorth's alone. The only place for theories in such a handbook is where the district is providing important evidence which bears upon one or other of them. Thus the author rightly gives some space to the great transported boulder of the Roslyn pit. His account of the gravels seems good and full: probably some of it is original, but ought not a reader to be enabled to know? Should not the statements of a handbook be either matters of universal acceptance, or else opinions of

named authors whose names give weight to (or, take it away from) their statements?

An appendix contains a valuable list of books and papers bearing upon the county. The volume is printed as the Cambridge University Press can print. Errata are very few. It is a book likely to assist geologists, but not likely to create them. We have no right to expect the illumination of genius and we do not find it. Dr Bonney in the preface to his sketch of 'Cambridgeshire Geology,' hoped that it might be superseded by a Geology of the Valley of the Cam, to form a companion to that which Phillips executed for Oxford; and hoped that the present Professor would take it in hand. These hopes are not fulfilled.

A Student's Petrology

Petrology for Students. An Introduction to the Study of Rocks under the Microscope. By Alfred Harker, M.A., F.G.S. Second edition, pp. viii+334. 75 figures. Cambridge Natural Science Manuals. Cambridge: University Press, 1897. Price 7s. 6d.

The first edition of Mr Harker's "Petrology for Students" took rank as the most practical and best of the elementary English text-books on that science, and as perhaps the most useful students' book in the Cambridge "Natural Science Manuals." The present edition may be expected to be even more useful, for though only 27 pp. longer, it has been extensively revised and considerably improved. The references to foreign rocks especially have been largely increased, which in a general sketch of the subject is a step in the right direction. The author ingeniously overcomes the difficulty of dealing with many rock names, the value of which is doubtful, by not using them in the text, but quoting them in the index with a reference to the page where the rock is described. Thus, for instance, Monchiquite is given in the index with a reference to p. 143, where the rock is described as a type of ultra-basic lamprophyre, but the actual term is not used. The "yogoite" of Weed and Pirsson, the "grorudite" and "akarite" of Brogger, and the "litchfieldite" of Bayley, references to which are added in this edition, are all thus treated.

Another improvement is the adoption of the name "Hypabyssal" rocks instead of "Intrusive" rocks for the author's second group, in which are included the acid intrusives, porphyrites, diabases, and lamprophyres; for there can be no doubt that many of both plutonic and volcanic rocks are sometimes intrusive.

Northern Spain

In Northern Spain. By Hans Gadow. 8vo, pp. xvi+421. London: A. & C. Black, 1897. Price 21s.

In this entertaining volume Dr Gadow has combined a general account of two extended journeys through little known parts of northern Spain, with a valuable synopsis of his observations on the natural history of the country. He also makes special reference at times to practical matters affecting a traveller; so that his narrative will not only be read with interest by those who are unable to imitate

his experiences, but will also prove of much value to those who contemplate similar wanderings.

The greater part of the work is occupied with the narrative, which is illustrated by many beautiful photographs and several outline sketches. Then follows a brief account of the dolmens or cromlechs of the province of Alava, with some sketches and plans, and this chapter concludes with a list of all the known dolmens and similar prehistoric remains in Spain and Portugal. The next 40 pages are devoted to a condensed account of the history of Spain, with special reference to the northern provinces. The final two chapters, occupying 50 pages, are those to which the naturalist will eagerly turn, as containing Dr Gadow's valuable notes on the fauna and flora of the country. These include not only observations on the organisms themselves, but also a record and discussion of the native names for the various plants and animals. These names prove of exceeding interest, for it appears that the languages of the Basques and Celts, the Romans, the Goths, and Arabs have all contributed to the vernacular nomenclature.

As the result of combined observations up to the present time, Dr Gadow points out that all the animals now confined to the Spanish Peninsula have their nearest allies in other European countries, except a species of ichneumon of the African genus *Herpestes*. This is supposed by some naturalists to have been introduced by the Moors to kill rats and mice; but it is specifically distinct from any ichneumons now living elsewhere, and there are other considerations which suggest that it is truly indigenous to Spain. Very little is known of the Pleistocene fauna, except from the caverns of Gibraltar; but there are many indications of a former glaciation of the mountains of the Peninsula, and Dr Gadow himself has contributed an interesting additional proof of the former prevalence of a colder climate in this part of Europe by the discovery of mummified bodies of the lemming in some caverns to the north of Santarem.

The collection of plants identified by Mr Burkill comprises only about eighty species, and Dr Gadow's notes only relate to seventy more; but several hitherto recorded forms are added to the flora of the Asturian and Cantabrian mountains.

A short appendix contains further etymological notes and a list of works referred to in the text, while the volume concludes with a useful map and index.

Practical Botany

Das Kleine Botanische Practicum fur Anfänger. By Dr Eduard Strasburger. Third revised edition. 8vo, pp. viii+246, with 121 woodcuts. Jena: Gustav Fischer, 1897. Price 6 marks.

Teachers and students will welcome the third edition of Prof. Strasburger's "Practical Botany." The arrangement is the same as that of the two previous editions, but the results of experience gained in the four years that have passed since the appearance of the second are embodied in the third. The most noteworthy alteration is the reduction in the number of objects to be examined in each lesson, the author wisely remarking that it is better for learners to gain a more complete knowledge of fewer things. Even as it stands at present,

there is matter for several hours' diligent application in each division, and the student who has honestly worked through the thirty-two chapters will have a very fair knowledge of microscopic technique, and of the structure of a representative series of plants. It is often stated that illustrations are out of place in a book dealing with practical work. The excellent figures (prepared by the author himself) form one of the most useful features of the book, and give the lie to such statements; but it is only because they are not mere diagrams, but actual pictures of what the student may see in his own section when he has attained the requisite skill in the use of his razor. Those who have tried both know how much easier it is to work out a section with the help of a picture than by following a mere description, however carefully drawn up. Another good point in Strasburger's arrangement is its commencement with the study of the cell and its contents, followed by a general introduction to plant structure before passing to a more special investigation of individual types. The type system is in fact throughout subordinated to the general idea, and herein lies the chief superiority of Strasburger's over our English introductions to the practical study of plants. It is matter for surprise that, considering the number of our teachers of botany who have been attached to Strasburger's laboratory at Bonn, there is no good elementary book in our language on the same lines.

New Serial

In 1897 Messrs Georg et Cie of Geneva published the first volume of the *Annuaire du Conservatoire et du jardin botanique de Genève.* It is edited by Dr John Briquet, and sold for 5 francs. The volume contains an account of the garden and Delessert herbarium for 1896, and various scientific papers.

Recent Serials

Professor G. S. Boulger will take over the editorship of *Nature Notes* with the April number. The Selborne Society could scarcely have found a more useful officer.

As some of our readers may be meteorologists, we should draw attention to the March number of *The Photogram*, which contains an article on the "Photography of Clouds," illustrated by admirable reproductions of photographs.

With the present year, the excellent little Norwegian Magazine *Naturen*, blossoms out in a new cover of very artistic design. Nos. 1 and 2, which are issued together, contain an article by Prof. R. Collett on "The Mode of Life and Occurrence of the Beaver in Norway," illustrated by figures and two plates. Following it is a careful account by L. Schmelck of some researches on the water from ice-fields and glaciers, discussing the cause of the green colour of the ice-water.

The *American Naturalist* is now published by Messrs Ginn & Co., of Boston, and its January number appeared in a new cover of pleasing appearance, and printed on highly calendered paper of somewhat

larger size. The aims of the *American Naturalist* appear, from an editorial note, to remain very much what they were before. They are precisely the same as those of *Natural Science*, and although we cannot fail to miss the guiding hand and original criticisms of the late Prof. Cope, we trust that the new editors of the *Naturalist* may be more successful than ourselves in approaching their ideal. The number in question begins well with a Synopsis of Recent Progress in the Study of Graptolites by Dr R. Ruedemann.

The new "Geological literature added to the Geographical Society's Library during the year ended 31st December 1897" is a bulky volume of 196 pp. As this is published at two shillings, and as the Geological Society gets a large proportion of the publications on geology during any given year, one has practically a "Geological Record" for quite a trifling sum. Moreover, as ninety pages out of the one hundred and ninety-six are allowed for an index to the titles appearing in the first half, our readers can readily understand the value of such a publication. We think a word of thanks is due to the assistant librarian for his compilation, a second to the assistant secretary for his editing, and a third to the society, who certainly provide us with a very respectable if incomplete guide to the year's geology.

No. 2 of the *Bulletin of the Liverpool Museum* was published Feb. 28, and continues the catalogue of birds in the Derby Museum. Dr Forbes also describes, under the name of *Necropsar leguati*, a new species of bird from the Mascarene Islands, supposed to be already extinct. This bird was noticed somewhere about 1730 by some marine surveyor (name unknown), who was sent to Mauritius by the Compagnie des Indes; but the bird to which he refers was unknown to scientific men until Dr Forbes discovered a perfectly preserved skin, among the specimens of *Hypsipetes*, in the Derby Museum, where it had lain unnoticed for nearly fifty years. A sub-fossil species of the genus *Necropsar* was described by Dr Günther and Sir E. Newton in 1874. This number concludes with an illustrated account of some highly ornamented metal-work from Benin, brought from that city by the punitive expedition of 1897.

Further Literature Received

Lessons with Plants, Bailey: Macmillan & Co. Grundzüge d. Geogr.-morph. Methode d. pflanzensystematik, Wettstein: Fischer, Jena. Naturalists' Directory: Upcott Gill. L'Algérie, Battandier et Trabut: Baillière, Paris. Catalogue of Madreporaria, vol. iii., Bernard: Brit. Mus.

Studies from Yale Psychol. Lab., vol. iv. Odours and Tastes of Surface Waters, Jackson & Ellms: *Technol. Quarterly*. Report Lanc. Sea-fisheries lab., 1897. On a Quartz Barytes Rock, Holland: *Rec. Geol. Surv. India*. Ann. Rep. Field Columbian Mus., 1896-7. Fishes and Reptiles from Somali-land, Meek & Elliot. *Field Columb. Mus. Publication*, No. 22. Ann. Rep. Museum, Bolton, 1897. Aus der 22 Jahresversammlung deutsch Ornitholog. Gesell.: *Abh. Zool. Anthrop. Mus. Dresden.*

Actes Soc. Sci. Chili, vii. 4; Amer. Journ. Sci., Mar.; Amer. Nat., Jan.; Botan. Gazette, Feb.; Bull. Liverpool Mus. i., 2; Feuille des jeunes Nat., Mar.; Irish Nat., Mar.; Knowledge, Mar.; Literary Digest, Mar.; Naturae Novit., Feb., No. 3; Naturalist, Mar.; Nature, Feb. 24, Mar. 3, 10; Nature Notes, Mar.; Naturen, Jan. and Feb.; Pearson's Mag., Feb.; Photogram, Mar.; Plant World, Nov. 1897, Jan. Feb. 1898; Revue Scient., Feb. 26, Mar. 5, 12; Riv. Psichologia, 19, 20, 21; Science, Feb. 11, 18, 25; Revue, Sci. Nat. Ouest, No. 2, 1 Ap. 1897; Scientific Amer. Feb. 12, 19, 26; Mar. 5; Scot. Geogr. Mag., Mar.; Scot. Med. Journ., Mar.; Victorian Nat., Jan.; Westminster Rev., Mar.

OBITUARIES

SIR RICHARD QUAIN, BART.

BORN AT MALLOW, OCT. 30, 1816. DIED IN LONDON, MARCH 13, 1898

RICHARD QUAIN was the son of Mr John Quain, of Carrigoon, Cork, and a relation of Dr Jones Quain, the co-editor of "Quain and Sharpey's Anatomy." He spent the first twenty years of his life in Ireland, having been educated at Cloyne, and later apprenticed to a surgeon at Limerick. At twenty Quain came to London, and continued his medical studies at University College Hospital, where he was House Surgeon for five years. In 1840 he took the degree of M.B. at the University of London, winning the scholarship and gold medal in physiology. In 1842 he took his M.D. degree, being the only candidate to whom the gold medal and the certificate of proficiency were awarded; and within the next four years (before he was thirty) he was elected a Fellow of the University. He is best known as a specialist on diseases of the heart, chest, and kidneys, and was for many years consulting physician to the Hospital for the Diseases of the Chest at Brompton. In 1851 he was elected a Fellow of the Royal College of Physicians, of which College he had been a member for five years, and of which later he became the vice-president. In 1860 he was nominated a member of the Senate of London University, and in 1863 Crown representative on the General Medical Council. He was a most valuable member of this body, acting as its treasurer for some years, and was elected its president in 1891. In 1887 the Royal University of Ireland conferred the honorary degree of M.D. upon him, an example which was followed by Trinity College, Dublin, in 1890, while in 1889 he had received from Edinburgh University the title of LL.D. He was a Fellow of the Royal Society.

Of his numerous writings he will be best remembered by his "Dictionary of Medicine," edited in his leisure hours between 1872 and 1882, in which year it was published in a massive volume of 1800 pages. In 1852 he published his treatise on "The Fatty Diseases of the Heart." In 1872 he delivered the Lumleian Lecture on the "Diseases of the Muscular Walls of the Heart," and in 1885 the Harveian Oration on "The Healing Art in its Historic and Prophetic Aspects."

The death is also announced of Dr T. C. WINKLER, for many years curator of the Teyler Museum, Haarlem. He was much interested in the fossil vertebrate animals, of which he had a very fine collection under his charge. He contributed several articles on the specimens in the Teyler Museum to the *Archives* published by that institution.

He also wrote a popular work in the Dutch language on "The Vertebrate Animals of the Past," which we noticed in *Natural Science*, vol. viii., p. 347 (1896). Dr Winkler was elected a Foreign Correspondent of the Geological Society of London so long ago as the year 1874.

PROFESSOR WILHELM JOEST, whose death we recorded in February, died on Nov. 25, 1897, on the Island of Santa Cruz, Australia, while on an ethnological expedition from Berlin, aged 45.

The following deaths are also announced :—

N. ALBOFF, Russian botanist, at La Plata ; Dr DELMAS, geologist, at Castries, Aveyron ; the Portuguese explorer, Captain ROBERTO IVENS ; JEAN LINDEN, the botanist of Brussels, on Dec. 12, 1897, aged 81 ; Mrs NETTLESHIP, mother of the famous family of Nettleships, at Oxford, in February, aged 81 ; HEINRICH RIBBE, the entomologist, at Radebeul, near Dresden, on Jan. 19, aged 65 ; OSKAR VON RIESENTHAL, the ornithologist, on Jan. 21, at Berlin, aged 67 ; F. W. SEYDLER, botanist, at Braunsberg, aged 80 ; C. G. SOHST, the conchologist, at Hamburg ; IMBAULT HUART, French consul at Canton, well known for his geographical and ethnographical researches in Formosa, aged 40 ; Dr JOHANN VOLENTIN, of Frankfurt-on-Maine, while on an expedition in Patagonia for the National Museum of Buenos Ayres ; Prof. ALBERT ZIONMETER, the botanist, at Innsbruck, on Dec. 15, 1897, aged 49 ; GUSTAV ZIMMERMANN, the entomologist of Brüx, Bohemia, on Dec. 29, 1897, aged 66.

NEWS

The following appointments are announced :—G. C. Bourne, of New College, Oxford, to be lecturer in comparative anatomy in Oxford University ; Dr Pio Mingazzini to be professor of zoology and comparative anatomy at Catania ; Francesco Saverio Monticelli, as professor of zoology in the University of Naples ; Dr Karl Hürthle, of Breslau, to be professor of physiology and director of the Physiological Institute of that University in succession to the late R. P. Heidenhain ; William S. Carter to be professor of physiology in the University of Texas ; Dr Gregor Kraus to be professor of botany in the University of Würzburg ; Prof. Eugen Askenasy to be hon. professor of botany at Heidelberg ; Dr Magueene to be professor of plant-physiology at the Collège de France ; Dr Lüstner, of Jena, to be assistant at the experimental station for plant-physiology in Geisenheim ; Lewis V. Pirsson to be professor of physical geology in Harvard University ; Baron von Firks, of Mitau, to be assistant in geology in the Bergakademie of Freiburg i. S.

Dr John Murray has been elected a corresponding member of the Russian Geographical Society.

Miss A. Mallet has presented to the Geological Laboratory of King's College, London, a collection of minerals and recent shells.

Owing to the illness of his infant son, Dr Nansen was obliged to cancel his lecture engagements and return hurriedly to Christiana.

Under the will of Mr J. H. Harris, of Wagnesville, Ohio, the U.S. National Museum has received his collection of Ordovician fossils and of antiquities from that locality.

On March 17th, Prof. Boyd Dawkins delivered the 'James Forest' Lecture the Institute of Civil Engineers, speaking on "Geology in relation to Engineering."

After two years' exertion the Hornsey District Council has at last made arrangements to complete the purchase of churchyard, Bottomwood, for the sum of £25,000.

The Dublin Naturalists' Field Club, at a recent meeting, adopted a resolution urging the Boards of Primary and Intermediate Instruction to introduce natural science in Irish schools.

Mr C. W. Andrews, whose stay in Christmas Island, S. Java, is extended for the requirements of his researches, has forwarded five more cases of specimens of natural history objects to the British Museum.

The Lectureship in Geography at the University of Cambridge supported by the Royal Geographical Society has now been made a readership, with a total stipend of £200 a year. The Reader is Mr H. Y. Oldham.

The fifth International Congress of Hydrology, Climatology, and Geology, will be held at Liège this year, from September 25 to October 1, under the patronage of His Royal Highness Prince Albert of Belgium.

THE Ornithologischer Verein of Vienna has become a section of the Zoological and Botanical Society of that city, and its quarterly journal, *Die Schwalbe*, has been discontinued. The work, however, will be continued, and the results published by the Society.

A CAST of the skeleton of *Iguanodon bernissartensis* has recently been procured for the Oxford Museum by means of donations from the friends of the Museum and former students of the Department of Comparative Anatomy, and has been set up in the court of the museum.

Science announces that Dr Thomas Egleston, professor of mineralogy and metallurgy at Columbia University, has presented the Government of France with the sum of $5000, in aid of the mineralogical collection of the School of Mines at Paris, from which he graduated in 1860.

AMONG those who have received grants from the Elizabeth Thomson fund are Prof. John Milne 250 dollars, to aid in a Seismic Survey of the world; Prof. Bovaceni 288 dollars, for researches in colour photography; and Prof. Gustav Hüfner for the investigation of haemin and haematine.

THE German Antarctic Expedition Committee wishes to send a ship towards the South Pole, starting from or near the island of Kerguelen, under the direction of Dr Erich Drygalski. The expedition will winter in the Antarctic and make geological observations on the coast of Victoria Land.

THE Croonian Lecture of the Royal Society was delivered on March 17th by Prof. Pfeffer, professor of botany at Leipzig, who took for his subject "The Nature and Significance of Functional Metabolism in the Plant." Dr Pfeffer has received the honorary degree of D.Sc. in Cambridge University.

Science announces, in a particularly strongly-worded note, that "the person named Bowers from West Virginia" has been appointed U.S. Fish Commissioner. We sympathise with our contemporary, but dare not repeat its language for fear of international complications. The President's appointment has been confirmed by the Senate.

THE Annual Report of the Geologists' Association for 1897 shows a balance of £42, 15s. 11d. They stand possessed of £820, 16s. 10d. We are glad to note that £14, 8s. 4d. has been spent on the library, which means that the bulk of that amount has been paid for binding many of the loose serials and rendering them available to the members.

THERE has been founded at Cambridge University a Gedge Prize in Physiology, which is to be given for an original memoir, and is to consist of two years' interest on a capital of £1000. Candidates must have worked in the University laboratories during six terms, and be at least five and at most seven years' standing from matriculation.

A WASHINGTON Academy of Sciences has been established. According to *Science* its policy and functions have yet to be determined, but it appears intended to co-ordinate the action of the various science societies already existing in Washington, and especially to focus their energies when any question of public policy is concerned. In this way it will act after the manner of our own Royal Society.

PROF. T. M'KENNY HUGHES has completed his twenty-fifth year of office as Woodwardian Professor of Geology at Cambridge, and on February 26th he was presented with an illuminated address at a largely attended public dinner, at which Sir Archibald Geikie took the Chair. Among the speakers were Sir Henry Howorth, Prof. James Stuart, and Dr Hicks. On the following Monday, Prof. Hughes was presented with a silver loving-cup by his past and present students at Cambridge.

PROF. C. E. BEECHER, of Yale University Museum, has made for his class-use a series of enlarged models showing the development (embryology) of a typical member of the class Brachiopoda, and the structural peculiarities of ten of the most important genera. Under arrangement with Prof. Beecher Messrs Ward of Rochester, New York, have issued these models at the price of $60, or in an oak case, $70. The form taken as the type is *Cistella neapolitana*, but the price places these models beyond the reach of most teachers.

PRIZES of 600 francs are offered by the Belgian Academy for international competition. Among the subjects are :—Original Research on Digestion in Carnivorous Plants; Original Research on the Structure and Development of a Platode with a view of determining whether there are any phylogenetic relations between Plathelminthes and Enterocoela ; do the Schizophyta possess a nucleus ; if so, what is its structure and its mode of division ? Further information may be obtained from the Secretary to the Academy at Brussels.

SIR WILLIAM MCGREGOR is returning from the governorship of British New Guinea after ten years' continuous service in Australia's Equatorial Dependency, during which he has done splendid service as administrator, organiser, and explorer. Our contemporary *The Echo* reminds us that Sir William began life as house surgeon to the Royal Infirmary at Glasgow, and was first sent by the Government to the Seychelles. From there he was promoted to the Mauritius, and afterwards to Fiji. We agree with *The Echo* in thinking it will be difficult to replace Sir William McGregor.

THE Geologists' Association will visit Bridport and Weymouth for their Easter Excursion, April 7 to 12, under the guidance of the Rev. Prof. J. F. Blake ; they will go to Aldeburgh, Suffolk, for Whitsuntide, under Messrs Whitaker, Harmer and Ridley. The Long Excursion will be to Birmingham, Nuneaton, Dudley, Lickey, and Cannock, under the leadership of Professor Lapworth, Messrs W. W. Watts, W. J. Harrison, and W. W. King. If the Lapworth-Watts Excursion is anything approaching the famous excursion to Shropshire of a few years ago, then indeed will the Association have cause to be proud.

THE Seventh Meeting of the Australasian Association for the advancement of Science, held at Sydney early in January, was a great success judging from the reports in the Sydney *Daily Telegraph* and *Sydney Morning Herald*, which papers have been sent to us by Prof. Liversidge. There are over 600 members of the Association ; the sister Colonies were well represented, and there was a large attendance of visitors. A popular lecture was delivered by Prof. Baldwin Spencer, on "The Centre of Australia" ; while Sir James Hector discoursed on "Antarctica." The 1900 meeting will be held at Melbourne, under the Presidency of Mr R. L. J. Ellery, the Astronomer.

THE expected has happened. The collection of fossils made by Mr W. F. E. Gurley is now for sale. It will be remembered that when Mr Gurley was State Geologist of Illinois, he utilised the official publications of his department for the description (in conjunction with the late Mr S. A. Miller) of an extraordinary number of new species, the types of which were in his own private collection. We pointed out at the time that he thus greatly increased the commercial value of the collection at no cost whatever to its owner. We have every reason to believe that Mr W. F. E. Gurley is an honourable man, and we have no doubt that before parting with his collection he will present these type-specimens to the State of Illinois.

ARCTIC explorers are springing up on all sides. Another attempt on the Pole is to be made by Captain Bernier, who appears to be a native of Quebec. He intends to go by ship to the point north of Siberia where the *Fram* crossed 80° E. Here he will take to the ice with eight men, fifty dogs and fifty reindeer, sledges,

kyaks, and a portable boat. Captain Bernier is applying to the Canadian Government for assistance, and is backed by the Geographical Society of Quebec.

The Duke of the Abruzzi, nephew of King Humbert, accompanied by Drs Gonella and Defilippi, intends to visit Spitzbergen during the approaching summer, and in 1899 to visit Franz Josef Land, whence an attempt will be made to reach the Pole by means of sledges and dogs. The latter are to be obtained from Greenland.

THE Annual Report of the Geological Society shows that the Society stands possessed of £13,226, 3s. 8d., exclusive of the library, collections, furniture, and stock of publications. We are glad to see that the Council contemplate spending some of their riches on the "Publication of Hutton MS." We note that electric light in 1896 cost 11s. 6d., and in 1897 £13, 3s. 6d., and suppose we may take the 1897 amount as a fair and reasonable yearly amount for lighting.

PROF. HADDON, whose expedition to Torres Straits we announced last July, left Tilbury on Thursday. Before returning the members will visit New Guinea and Borneo, and carry out a thorough scientific exploration of their mineral and other wealth. We are indebted to the *Echo* for the following details of the expedition. Prof. Haddon is accompanied by several well-known scientists, each of whom will devote his attention to the special branch of study of which he is a master. The leader of the expedition has decided to study the decorative art of the various tribes whom they are about to visit, while native music will be a subject taken up by Dr C. S. Myers, Caius College, Cambridge, who, as part of his equipment, has taken out a phonograph with him, as well as a cinematograph for reproducing dances and other native ceremonies. The well-known Polynesian scholar, Mr S. A. Ray, will direct all observations with regard to the different languages and folk-lore of the tribes, while Dr Rivers, St John's College, Cambridge, will study their beliefs and superstitions. Dr W. M'Dougall, Dr Seligmann, of St Thomas's Hospital, Mr A. Wilkin, of King's College, will make notes on botany, geology, zoology, &c. The object of Prof. Haddon is to obtain full information as to the physical characteristics, customs, amusements, songs, and condition generally of these tribes, and to supplement these observations with collections of scientific interest.

NOTICE

TO CONTRIBUTORS.—All Communications to be addressed to the EDITOR of NATURAL SCIENCE, at 29 and 30 Bedford Street, London, W.C. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

TO THE TRADE.—NATURAL SCIENCE is published on the 25th of each month; all advertisements should be in the Publishers' hands not later than the 20th.

TO OUR SUBSCRIBERS AND OTHERS.—There are now published TEN VOLUMES OF NATURAL SCIENCE. Nos. 1, 8, 11, 12, 13, 20, 23, 24 being OUT OF PRINT, can only be supplied in the set of first Four Volumes. All other Nos. can still be supplied at ONE SHILLING each.

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